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Psychological Variables In Human Cancer

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During the eight years from 1939 to 1947, when I served as Director of the Rorschach Institute before it blossomed into the Society for Projective Techniques, I faced many difficult tasks, as Dr. Bellak so friendly hinted, but never before one as difficult as this, presenting a presidential address. This task is especially formidable in terms of tradition; if you think back to the first years of our organization, for instance, the very first presidential address was Morris Krugman's "Out of the Inkwell"; the second was Marguerite Hertz's "Twenty Years After," shortly after that Marie Rickers presented her address called "Some Theoretical Considerations Regarding the Rorschach Method." These papers are even now classics among the 3,000 publications on the Rorschach method which have accumulated since then.

As Dr. Bellak pointed out, five authors of such presentations are here in the room and make me feel very anxious as to how my presentation will compare with theirs.

It is quite natural to expect from presidential addresses more than just an interesting talk; more than just some interesting material. All these presentations were characterized by a gleam in the eye of the speaker who told of wide horizons, of wise overviews of a large sector of the field.

Following my non-conformist tradition I felt, oddly enough for my usual point of view, that I would rather start this presidential address from a specific piece of research.

Those of the present former presidents who have presented some research material, like, for instance, Theodora Abel with her Chinese records used it as a point of departure for a larger theoretical consideration. I hope the selection I have made will also serve as a departure into wider horizons. But I will leave it to you to judge the significance of what I have to say.

The beginning of this project goes back about seven or eight years. A couple of physicians at the Veterans Administration Hospital in Long Beach, California—the head of the cancer department and a biophysicist who worked very closely with him—got interested in some extensive research work with cancer patients in what we nowadays might call the symbiosis between the cancer patient and his cancer. They left aside all the difficult problems connected with the genetic aspects of cancer, and concentrated on one very well circumscribed problem, namely, the mysteries of the growth rate, the rapidity or slowness of the growth of cancer. This has been observed as a problem for many, many years and there are indications that there is something peculiar about it.

This problem has even what you might call a sentimental angle. Very often physicians have observed that the cancer seems to be particularly successful in some of the patients whom they appreciate most as human beings.

There may always be a little retroactive halo effect that you feel a person was worthwhile when he dies, according to the Latin principle "de mortuis nil nisi bonum"—not to say anything bad about the dead—but still there is quite some evidence that this is not only a sentimental illusion.

There are indications, for instance, that in mental hospitals cancer has a much harder time than outside. Not only is cancer proportionately slower

[1] Presidential address delivered at annual meeting of Society for Projective Techniques, August 31, 1957, New York, N. Y.

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growing among schizophrenics, but the schizophrenics seem to live much more comfortably with their cancer than other people do. I don't know whether there are exact statistical data, but this is a general observation that seems pretty well established.

In order to investigate this problem scientifically, the first thing to do was to objectify the concept of the rate of cancer growth. How can you say, or where do you draw the line in order to say that a cancer is fast or slow in growth?

Now, these two physicians, Dr. Frank Ellis and Dr. Philip West, used a fairly simple way of approaching this knotty problem. They got hold of all available U. S. statistics about the life duration of cancer patients. It is well-known that this depends first on the kind of cancer from which a person suffers. For instance, acute leukemia, which takes mostly children as its victims, is probably of all cancers the most rapid type, while on the other hand cancer of the breast is not only least terrifying because it is most susceptible to surgical removal, but, even besides that, seems to be one of the slowest-growing cancers.

So, first of all, if you want to decide whether a cancer grows slow or fast, you have to decide it for each type of cancer growth separately.

It seems, if you take these U. S. statistics as a basis, that there is a certain distribution of longevity or survival time for each type of cancer. You determine for each cancer how many people are still alive who suffer from this cancer from the time when it was discovered (which is uncertain in many cases), in one year, in two years, in three years, in four years, in five years, and so on. What they did is simply to take a crude measure for separating slow and fast cancer growth by determining for each kind of cancer the time period in which only 25 percent of the patients with this particular kind of cancer had died. Then they determined the second point—when 75 percent of all patients with this particular cancer had died and only 25 percent were still alive. They decided to sacrifice, in an effort to objectify the concept of fast and slow kinds of growth, 50 percent of all possible cases and exclude from their research all cases whose time of death fell between the 25th percentile and the 75th percentile of duration of survival.

They decided that anybody who dies earlier than the first 25 percent of the patients with a specific cancer had unquestionably a fast cancer growth; and anybody who is still alive after 75 percent of all patients with the same type of cancer have died showed slow cancer growth.

This takes care, also, of one other factor which may or may not have a strong influence on the cancer personality picture, and that is the age of the patient. There are certain types of cancer with a very specific age distribution, for instance, acute leukemia. Thus you compare each patient with his own age group. In those cases where the cancer seems to be rather without a specific age distribution, it seems likely that age doesn't seem to be decisive.

The next point was, how do we go about finding out whether this medical fact—that they had a fast or slow-growing cancer—could be related to personality.

The whole research project and a great deal of the original data are published in a book which has the same title as my talk, "Psychological Variables in Human Cancer," which was published in 1953 by the University of California Press.

My own contribution to that book consists of three pages in one place and one page in another. I am therefore not repeating what I have already published, because all that I published were a few preliminary remarks in a symposium we had in October of 1953, without having had an opportunity to work over the data on which these preliminary impressions were based.

I may say, just for the sake of exactness, that I did talk once before in February 1956 at the Northern California Chapter of the Society for Projective Techniques about this material, and it was a very interesting discussion where Dr. Harrison Gough reported about some very meaningful parallel results with other psychological tests in TB patients. But first I have to tell you how I got into the whole business myself.

In connection with this research two of our Ph.D. candidates wrote their dissertations. They used other techniques but also collected Rorschach records of these slow and fast cancer patients. Then a routine situation developed which I have come across quite a number of times. They had the Rorschachs on these cancer patients and they didn't know what to do with them.

Since I was coming at that time every week, later every two weeks, as a consultant to this particular hospital, they said, "Well, you come here anyway. You could do some work. Why don't you spend an hour and a half of each visit and have a look at these Rorschach records?"

The only approach I know to such a problem is a direct empirical one: "All right, let's get started. You pick me three records of patients who have fast-growing cancers and three records of patients with slow-growing cancers and I will look at them and see if I see any difference."

I looked at these six records in one of our first sessions and I got some ideas in what way they might be different from each other. In the second half of my presentation, I'll discuss what this way was, so I won't go into it at this moment.

We had used up six patients for this preliminary study and then I looked at the Rorschach records of twenty-four patients blindly. I knew they were cancer patients, to be sure. I knew, also, their sex and age. They were all men, except one, by the way. But that is all I knew. I also did not know what kind of cancer they had. Interestingly enough, that wouldn't have made much difference to me because I didn't know enough about it when we started this research.

I can give you very simply the results of this experiment, if you want to call it that. I may say, by the way, that among these twenty-four cases, there were not only postdictive cases, there were also three or four newly-arrived cancer cases where none of the doctors could foretell whether they would be fast or slow. So there we made real predictions not postdictions.

Out of the twenty-four patients I postdicted or guessed correctly that nine of these twenty-four had a fast-growing cancer. I also guessed correctly that ten of these twenty-four had slow-growing cancer. I could not decide which it was in one case. And in each case I made two mistakes; two fast-growing cancers I called slow and two slow-growing cancers I called fast.

The statistics showed that this could not have been a pure chance result. (Chi-square is 6.042 corrected for continuity; $P < .02$)

Now, we had one other criterion to use, that of life or death itself. Five years ago all this work had gone on, just recently I got the idea, "Now let's see what happened to these patients in the meantime." You understand that whether they are still alive or not does not determine whether there was a fast or slow-growing cancer because if a patient has lived beyond the 75 percent of his quota, even if he dies, which we all eventually do, he still remains a slow-growing cancer patient. But if any of these patients are still alive we are sure that they were in the slow-growing group. As a matter of fact, in December 1956, of the ten whom I had predicted or postdicted correctly as having slow-growing cancers, four were still alive and none of the others were living.

I can give you some other figures about the experiment. The MMPI

was also used for such postdictions. In 44 cases, 21 were correctly identified as fast-growing cancer, 13 correctly identified as slow-growing; five of each group wrongly identified. It is a larger group of people, therefore the statistical significance is on a .001 level as far as MMPI is concerned. (Chi-square = 12.356)

In fifteen cases we had postdictions from the Rorschach and the MMPI. Eleven of these patients were correctly recognized by both the Rorschach and the MMPI; two were incorrectly recognized from the MMPI, correctly from the Rorschach; two correctly recognized from the MMPI and incorrectly from the Rorschach; and only one case of the 15 mis-diagnosed by both techniques.

These figures are supplemented by two later experiments, if you want to call them that. Last October at the M. D. Anderson Hospital in Texas a most interesting research conference was called of about forty psychologists and physicians from all over the United States, all of whom were actively engaged in cancer research from the psychological point of view. They asked me at the conference to perform the same experiment with two of the patients at the hospital there, this was a particularly ticklish experiment because they had then only melanoma patients in their research files and slow-growing cancers are very rare in this group. One of my faculty colleagues was present also at this experiment and I don't think he was so much impressed by what I did, but he was impressed by the reaction of the other thirty-eight people. He happened to be in charge of our research seminar so he asked me whether I would be willing to stick my neck out in front of 150 students and 50 faculty colleagues. So I said, "Chop it right off." They selected three cases again from the files of the Long Beach Hospital whom I had not seen before and I identified all three correctly. In these subsequent experiments I therefore identified correctly four out of five.

You may assume no matter how I did it, whether I did it by ESP or anything else, that this might be an interesting result in itself. What could it mean? If anybody, no matter by what means, can, merely from a Rorschach record, say whether a person's relationship to his own cancer is such that the cancer grows fast or that the cancer grows slow, what possible inference can you draw from that?

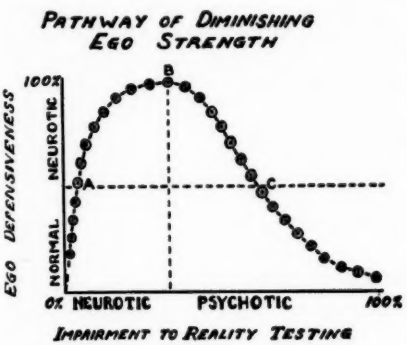
As far as I can see, there is only one possible inference, if we in the light of the statistical significance agree that this is not just a fiction but a real fact. However, in order to discuss that with you I have to explain to you first how I did it, or at least how I think I did it.

In order to perform this job I turned back to a conceptual scheme which had been slowly crystallizing in my mind for about two years preceding this experiment. This was the first time that I actually used this scheme as a research tool and I felt very much reassured by it.

I called this peculiar pseudo-statistical or quasi-statistical scheme, with my disrespect for nomenclature, "The Pathway of Diminishing Ego Strength." Assuming we had some kind of instrument with which we could accurately measure anybody's ego strength and we applied it to a random sample of a large population, let us say a million people—as long as we don't have to do it we can be generous—we could rank them from the person with the greatest ego strength all the way down to the person with the least ego strength. Then we take this long line of a million people and fit it into a scheme of two axes pictured on next page.

The vertical axis represents the amount of vital energy each of these people consumes in the defense of an insecure ego (level of ego defensiveness).

Quite arbitrarily, from the point



of view of this concept we grant generously to everybody that he can show 50 percent of the maximum amount of ego defensiveness and still have a normal adjustment.

People who use less than 50 percent of whatever vital energy can be invested in ego defensiveness are located from O to A and people who use more than half of the available amount of vital energy for ego defensiveness are above this point, "A."

At this point we have to make an important distinction between "adjustment" and "ego defensiveness." Below point "A" the two concepts are practically indistinguishable. Above point "A" we'll find many people who show an excellent outer adjustment—the very people who give rise to the sentimental aspect of our problem—but who pay too high a price in ego defensiveness for such achievements. Along with these well adjusted, but insecure people we find between point "A" and point "B" all kinds of overtly neurotic people.

The horizontal axis of the illustration represents loyalty to reality. How faithfully does a subject adhere to reality as it is, or how deficient is his reality testing? In this scheme we can describe the degree of impairment of reality testing from complete loyalty to reality to the point where there is no more real contact with reality.

Now, the zero point in reality testing and ego defensiveness in the scheme (where both axis cross) is an utopian point. It would locate those people who can take reality absolutely unflinchingly, without any need to defend their ego, without ever getting afraid of it. There are no such people, you know that. But theoretically we can say this is "the ideal type."

You could line up from this ideal point all the people who don't overinvest in ego defensiveness, whom we call normal. They can afford to be quite loyal to reality. They don't have to distort reality. Then we come to the point in our long line of persons from the strongest to the weakest ego to people who do overinvest in ego defensiveness. The illustration shows that you can invest a good deal in ego defensiveness and still stay pretty close to reality.

At point "B" we come to the point of diminishing returns. If you approach the point where you have invested all the vital energy you can in ego defensiveness, something gives way. Usually I say it's like the principle of butter and cannons. If you over-invest your economic strength in armament then you run out of butter. It seems a point where the overinvestment in ego defensiveness makes it impossible to retain loyalty to reality. You get a pretty sharp break where loyalty to reality diminishes rapidly. Strange as it seems, at that point where the ego structure starts to crack, suddenly the amount of vital energy invested in ego defensiveness diminishes.

At point "C" we come then to the strange phenomenon of people who are really quite sick but on a superficial look at their Rorschach records you have the feeling, "Oh, they are not much more uncomfortable than a fairly normal person." From about this point on in the scheme there is no longer anything to defend; the ego becomes dissociated and no more systematic effort in defensiveness can be recognized.

This is nothing but a schematization of something which I think every-

body knows. There are limitations to the scheme that I want to point out. I don't think it fits more than 80 percent of possible subjects.

Let us take just one possible large exception. There are various kinds of people whom I always describe as having a "floating island" ego. They have shallow roots for their ego. Hypomanic people, psychopathic people, and a number of other categories form this group. When they get under great stress, symbolically speaking their ego seems to pull up its roots and floats on the tide. They float away. And when the stress is over they settle down again and are the same as before. You don't have the feeling their ego becomes fragmented, as in the other cases. But most of the normals, neurotics, and psychotics, I would say about 80 of any hundred people, would fit somewhere into this scheme.

We have to make one more step. One of the reasons why I used these two particular components of ego functioning, the general level of ego defensiveness and reality testing, is probably that these two components are relatively clearly reflected in Rorschach records. The general level of subjectively-felt discomfort in dealing with the emotionally loaded stimuli of shading and color seems to be a good yardstick for the level of ego defensive investment.

The more a subject shows any of the myriad kinds of discomfort in handling, in using, or not using shading and color, the more subjective discomfort you see, the higher you have to rate him on ego defensiveness. And, if you investigate the form level, the qualitative aspects of the whole thought process, you can determine how far from the ideal point of loyalty to reality the subject is.

When I looked at these first six preliminary experimental subjects there was no question that every one of the people with the slow-growing cancer was, in his level of discomfort, below this line, A-C. And even though none of them was clinically a psychotic, they had developed a relationship to reality which you can only describe with the simple phrase, "They didn't give a damn." Their whole attitude was, "Why should I care?" and they were very sloppy in their organization of their responses. They changed and shifted around in their responses. They didn't have the very bizarre kind of responses which you find in many schizophrenics but, in comparison with the kind of intelligence which the whole Rorschach showed, they were not making any effort to be loyal to reality. They were all to the right of point "B" and at the same time they remained below line A-C in terms of ego defensive investment.

And the three fast cancer patients were all in the quadrant A-B without any question. They were all people who tried very hard to be loyal to reality and paid a terrible price for it.

This is the first hint of what the sentimental aspect of the whole problem is about, why a "prince of a fellow" dies so fast from cancer. It may not indicate whether these people were "good people" or not. It may simply indicate what price these patients, for whom the doctor always feels extremely sorry, had to pay for being "good"; that they overinvested too much ego defensive energy in their attempt to be good and loyal.

I can only say that my errors were mostly in this area between point "B" and point "C." In other words, in our clinical nomenclature we would have to say these were all people, whose fragmented ego defenses placed them somewhere between psychosis and neurosis.

According to my conceptualization, you couldn't do better than chance in this quadrant B-C because you could never tell whether the beginning fragmentation of the ego defenses has reached a point where the ego has practically given up defending itself, and allowed the psychotic coping mechanisms prevail, or whether the "scar tissues," figuratively speaking, of

these fragmented ego defenses create the kind of discomfort which uses up vital energy even faster than a well-compensated, well-functioning character neurosis.

If we leave this out for a moment, it seems that the chances of guessing correctly, if you are sure that a person belongs in this quadrant A-B or that the person belongs below this line A-C, is extremely high.

Let me add that fortunately among our samples we have also two patients who belong in quadrant O-A, people who were amazingly well adjusted not just in a superficial behavioral sense, but amazingly relaxed, amazingly mature.

I know one of these people personally. She has had Hodgkin's Disease now for over twenty years and occasionally she gets sick again but responds very quickly to X-ray treatment. She is a very extreme case in this category.

I had a very interesting experience last night. I met here one of my colleagues and we talked about whether he wants to come here tonight and listen. Then he said, "You know that I am a cancer patient." I said I had no idea. I hadn't seen him in quite a few years. He developed a very severe intestinal cancer five years ago, which was unfortunately not recognized. He says the doctor, when he went to him and talked about his complaints first, said to him, "Oh, that's psychosomatic." He said, "I'm a psychologist myself. I know what it is, it feels different." He went on a trip and he almost didn't make it home because the cancer had developed to such an extent that he nearly died on the way. He was immediately operated when he came home and this is now five years ago and as far as he can tell there is no new metastatic development. And this is a man who I would say, from the way I know him, is about as undefensive as I know any of my colleagues to be.

As I mentioned earlier, among the twenty-four cases which formed the basis of our original research project were four "misses" as far as the predictive or postdictive task of the Rorschach technique is concerned. In one of these misses the Rorschach record gives the impression that the patient had a slow-growing cancer while the medical indication put him clearly in the "fast" category. The rather dramatic course of events in this case may be more instructive than any examples we have covered so far. Therefore, I am inserting here the report of these events as given to me by one of the patient's physicians, Dr. Philip West, in a personal communication.

"Mr. Wright had a generalized far advanced malignancy involving the lymph nodes, lymphosarcoma. Eventually the day came when he developed resistance to all known palliative treatments. Also, his increasing anemia precluded any intensive efforts with X-rays or nitrogen mustard, which might otherwise have been attempted. Huge tumor masses, the size of oranges, were in the neck, axillas, groins, chest and abdomen. The spleen and liver were enormous. The thoracic duct was obstructed, and between 1 and 2 liters of milky flued had to be drawn from his chest every other day. He was taking oxygen by mask frequently, and our impression was that he was in a terminal state, untreatable, other than to give sedatives to ease him on his way.

"In spite of all this, Mr. Wright was not without hope, even though his doctors most certainly were. The reason for this was, that the new drug that he had expected to come along and save the day had already been reported in the newspapers! Its name was "Krebiozen" (subsequently shown to be a useless, inert prepartion).

Then he heard in some way, that our clinic was to be one of a hundred places chosen by the Medical Association for evaluation of this treatment. We were allotted supplies of the drug sufficient for treating twelve selected cases. Mr. Wright was not considered eligible, since one stipulation was that the patient must not only be beyond the point where standard therapies could benefit, but *also* must have a life expectancy of at least 3, and preferably 6 months. He certainly didn't qualify on the latter point, and to give him a prognosis of more than 2 weeks seemed to be stretching things.

However, a few days later, the drug arrived, and we began setting up our testing

program which, of course, did *not* include Mr. Wright. When he heard we were going to begin treatment with Krebiozen, his enthusiasm knew no bounds, and as much as I tried to dissuade him, he begged so hard for this "golden opportunity," that against my better judgment, and against the rules of the Krebiozen committee, I decided I would have to include him.

Injections were to be given three times weekly, and I remember he received his first one on a Friday. I didn't see him again until Monday and thought as I came to the hospital he might be moribund or dead by that time, and his supply of the drug could then be transferred to another case.

What a surprise was in store for me! I had left him febrile, gasping for air, completely bedridden. Now, here he was, walking around the ward, chatting happily with the nurses, and spreading his message of good cheer to any who would listen. Immediately I hastened to see the others who had received their first injection at the same time. No change, or change for the worse was noted. Only in Mr. Wright was there brilliant improvement. The tumor masses had melted like snow balls on a hot stove, and in only these few days, they were half their original size! This is, of course, far more rapid regression than the most radiosensitive tumor could display under heavy X-ray given every day. And we already knew his tumor was no longer sensitive to irradiation. Also, he had had no other treatment outside of the single useless "shot."

This phenomenon demanded an explanation, but not only that, it almost insisted that we open our minds to learn, rather than try to explain. So, the injections were given 3 times weekly as planned, much to the joy of the patient, but much to our bewilderment. Within 10 days he was able to be discharged from his "death-bed," practically all signs of his disease having vanished in this short time. Incredible as it sounds, this "terminal" patient, gasping his last breath through an oxygen mask, was now not only breathing normally, and fully active, he took off in his plane and flew at 12,000 feet, with no discomfort!

This unbelievable situation occurred at the beginning of the "Krebiozen" evaluation, but within two months, conflicting reports began to appear in the news, all of the testing clinics reporting no results. At the same time, the originators of the treatment were still blindly contradicting the discouraging facts that were beginning to emerge.

This disturbed our Mr. Wright considerably as the weeks wore on. Although he had no special training, he was, at times, reasonably logical and scientific in his thinking. He began to lose faith in his last hope which so far had been life-saving and left nothing to be desired. As the reported results became increasingly dismal, his faith waned, and after two months of practically perfect health, he relapsed to his original state, and became very gloomy and miserable.

But here I saw the opportunity to *double-check* the drug and maybe too, find out how the quacks can accomplish the results that they claim (and many of their claims are well substantiated). Knowing something of my patient's innate optimism by this time, I deliberately took advantage of him. This was for purely scientific reasons, in order to perform the perfect control experiment which could answer all the perplexing questions he had brought up. Furthermore, this scheme could not harm him in any way, I felt sure, and there was nothing I knew anyway that could help him.

When Mr. Wright had all but given up in despair with the recrudescence of his disease, in spite of the "wonder drug" which had worked so well at first, I decided to take the chance and play the quack. So deliberately lying, I told him not to believe what he read in the papers, the drug was really most promising after all. "What then," he asked, "was the reason for his relapse?" "Just because the substance deteriorates on standing," I replied, "a new super-refined, double-strength product is due to arrive tomorrow which can more than reproduce the great benefits derived from the original injections."

This news came as a great revelation to him, and Mr. Wright, ill as he was, became his optimistic self again, eager to start over. By delaying a couple of days before the "shipment" arrived, his anticipation of salvation had reached a tremendous pitch. When I announced that the new series of injections were about to begin, he was almost ecstatic and his faith was very strong.

With much fanfare, and putting on quite an act (which I deemed permissible under the circumstances), I administered the first injection of the doubly potent, *fresh* preparation—consisting of *fresh water* and nothing more. The results of this experiment were quite unbelievable to us at the time, although we must have had some suspicion of the remotely possible outcome to have even attempted it at all.

Recovery from his second near-terminal

state was even more dramatic than the first. Tumor masses melted, chest fluid vanished, he became ambulatory, and even went back to flying again. At this time he was certainly the picture of health. The water injections were continued, since they worked such wonders. He then remained symptom-free for over two months. At this time the final AMA announcement appeared in the press—"nationwide tests show Krebiozen to be a worthless drug in treatment of cancer."

Within a few days of this report Mr. Wright was re-admitted to the hospital *in extremis*. His faith was now gone, his last hope vanished, and he succumbed in less than two days."

Mr. Wright's Rorschach record was obtained before his transformation from optimism to pessimism took place. It reflects the picture of a personality with what I called previously a "floating island ego organization." This is reflected in his actual behavior and the great ease with which he followed first the suggestions of the drug advertisement and later on the deliberate experimentally motivated suggestion of his doctor without any sign of defensiveness or even criticalness. His ego was simply floating along and therefore left all available vital energy free to produce a response to the cancer treatment which seemed nothing short of miraculous.

Unfortunately this situation could not last since it was not reinforced by any deep-rooted personality center with a long-range point of view which could have counteracted the catastrophic effect of his disappointment about the drug. To use a symbolic analogy, while he was floating along on the surface of the water under the influence of his optimistic auto-suggestion or suggestion, he was transformed into a heavy stone and sank to the bottom without any resistance at the moment when the powers of this suggestion expired.

This analogy with the body swimming in water could also be used to express the whole core of our hypothesis figuratively (what we described previously as ego defensiveness): It could be described as a fear of life or death or both. Just as a person who is afraid of the water has a hard time learning to swim, so has a person who is afraid of life or death a hard time to fight against cancer. One can only utilize the carrying power of water optimally if one gives oneself trustingly to this element and then uses all acquired skills of moving around in water to maintain oneself and get to where one wants to go; thus the cancer patient has to have a deep-rooted confidence in life and an attitude to death which is not colored by terror but considers it simply as one of the forces one has to contend with in order to conduct a successful fight against cancer. The superficial success of Mr. Wright was also due to lack of ego defensiveness but could only last as long as reality testing could be avoided.

There are many other aspects to this material. One of the things I dream about is to go after all these patients given up by physicians and then "cured" by Christian Science practitioners. To study a number of these people who actually had metastasizing cancer and were cured would be an extremely interesting project.

Now I can come back to my idea, what it means; if there is any such connection between either the ego organization or the personality organization of the patient and the rate of cancer growth. The only possible explanation I can think of—but maybe you can think of others—is in a symbiotic relationship between the patient and his cancer. If a good deal of the vital energy the patient has at his disposal is used up in the defense of an insecure ego then the organism seems not to have the vital energy at his disposal to fight the cancer off and the cancer has easy going. If, however, a minimum of vital energy is consumed in ego defensiveness, then the cancer has a hard time making headway. This is the only explanation which makes sense to me.

Now this seems to me extremely important, as a symbolic expression of

the change in our psychosomatic thinking. Psychosomatic medicine started with an attitude that we wanted to find psychological causes for somatic events. Any attempt to follow the idea through with philosophical rigor and scientific exactness leads into difficulties. We would probably almost say today that the whole question of causation of somatic events by psychological factors in the literal sense of the word was probably the wrong question.

I would say that even in the whole philosophical problem of soma and psyche the same is true. The answer, however, is not an empty kind of parallelism. I think the most sensible terminology, even though the book which has been published about it has used not very convincing examples, is the concept of synchronicity by C. G. Jung. I leave it to you to follow this line of reasoning and develop it. I am very much impressed with this particular piece of research because it points so strongly in that direction.

One other theoretical aspect I want to mention in closing. When I published my first English book in 1942, "The Rorschach Technique," at the end of the foreword I expressed the pious hope that even though we didn't have, at that point, a personality theory, usable as a scientifically developed tool, we shouldn't put all our projective techniques on the shelf and wait hopefully until somebody presented us with a workable personality theory; that in using projective techniques we will slowly but surely come to a crystallization of a usable personality theory; that the development of a personality theory and the practical application of projective techniques have to go hand in hand, and the one cannot wait for the other.

I would say that this research, if anything at all, has very strongly confirmed and strengthened my belief that this is the way it will be.

Symposium: Research with Projective Techniques[1]

HERMAN FEIFEL, *Chairman*

I think a little background history is in order for our symposium today. As one of its objectives this past year, the Research Committee of the Society for Projective Techniques took upon itself the task of surveying the literature of 1956 for the purpose of selecting what it considered to be some of the best published articles involving research with projective materials.

The literature covered was limited arbitrarily to 6 journals which we felt encompassed the great majority of articles in this area. They were:

(1) *American J. Orthopsychiatry* (2) *J. Abnormal and Social Psychology* (3) *J. Clinical Psychology* (4) *J. Consulting Psychology* (5) *J. of Personality and* (6) *J. Projective Techniques.*

The final selection, by majority vote, and limited to 3 articles, was done by the following members of the Research Committee: Dr. Leonard Abramson, Dr. Henry David, Dr. Herman Feifel, Dr. Ralph Gundlach, Dr. Edith Weisskopf-Joelson, and Dr. Abraham Zeichner.

The three articles chosen were:

(1) "Past experience and present personality dispositions as determinants of selective auditory memory" by Anthony Davids, published in the September 1956 issue of Journal of Personality.

(2) "The TAT and anti-social behavior" by Kenneth Purcell, published in the December 1956 issue of the J. of Consulting Psychology.

(3) "The Negation TAT: a projective method for eliciting repressed thought content" by Richard M. Jones, published in the September 1956 issue of the J. of Projective Techniques.

This selection does not imply that these were the *only* publications during this period that the Committee felt were very good. There were, for instance, David Shapiro's paper "Color response and perceptual passivity" in the March 1956 issue of the J. Projective Technique, Vol. II of Bruno Klopfer's *"Developments in the Rorschach Technique,"* and others.

It has been heartening to see the progress made in the research use of projective materials. The more bizarre techniques are fading away; increasing attention is being devoted to underlying theory, e.g., relating Rorschach studies to general perceptual theory with promising use of psychoanalytic ego psychology; attention has been given to more suitable criteria and better validation procedures; and there has been more sophisticated use of the statistical armamentarium.

Nevertheless, there are still shortcomings. Surveying the mounting mass of quantitative research in the field one sometimes wonders, in evaluating it on a qualitative basis, whether, as John Rothney has suggested, the Red Queen's statement to Alice does not apply: "It takes all the running you can do, to keep in the same place."

The purpose of our symposium is to focus on what we regard as good research in the area — emphasize our own criteria and goals so-to-speak — by discussing and evaluating concrete articles. We hope, in this manner, to endorse certain conceptions of research and possibly indicate future directions of activity. We hold no illusions about our comments serving as edicts or sacrosanct promulgations. Rather, we view them in the nature of a springboard to a give-and-take sharing process with you. We look forward to your questions and comments later on.

[1] Presented at the annual meeting of the American Psychological Assoication, Sept. 1, 1957.

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Research with Projective Techniques[1]

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Our Chairman assigned us a presumptuous but highly rewarding task in asking each of us to select his nomination for the research-article-of-the-year, for 1956, in the field of projective techniques. Presumptuous because of having to select one among the many interesting, creative, and thought-provoking articles encountered in attempting to meet such a delimiting assignment, and highly rewarding for much the same reasons. In reviewing the projective techniques literature of 1956, I sought articles which were, of course, solid research, derived from and related back to theory, well designed and well executed. But over and above this, and perhaps with an element of personal bias, I looked for research that was novel in conception and intent upon pushing the frontiers of projective techniques and projective methodology into relatively neglected but seemingly important areas of personality assessment. With these criteria of selection in mind, my vote went to Anthony Davids' article: Past experience and present personality dispositions as determinants of selective auditory memory, Journal of Personality, September, 1956. In following up on the article, I became aware that the present article is but the latest of a series by Davids on this topic, the earlier ones providing necessary background and the present one carrying forward the earlier work in a systematic manner.

Projective techniques have relied almost solely on the eliciting and analysis of misperceptions and misapperceptions in the one sense modality of vision. Granted that most of our knowledge enters the mind through vision, with appeals through this modality augmented all the more with the advent of television, the auditory channels seem to have been strangely neglected in projective psychology. It is all the more strange when we consider that the first projective technique, word association, was an auditory one, that psychotherapy is conducted on the whole verbally, not visually (despite the recent re-emphasis upon non-verbal aspects of communication), and that auditory misperceptions are much the more common in and out of psychopathology. Suspiciousness and paranoia more commonly befall the deaf than the blind.

When I studied with and served as a student assistant to B. F. Skinner at Minnesota in the late 1930's, the "verbal summator" had already been devised and had been put into use, as the "tautophone," by Shakow and Rosenzweig at Worcester. Subsequently little was *heard* about this technique. The literature on other attempts at auditory projective techniques is indeed sparse, although there have been a few within the past few years. One such, that by Sydell Braverman, at the Woodrow Wilson, Virginia, Rehabilitation Center, in which I had occasion to participate as a judge, used a variety of auditory stimuli, both sound effects and verbal stimuli, the latter nonsense and contentual, enacted by professional actors and actresses. The results of her research are in process of analysis.

Davids' utilization of this sense modality is more circumscribed and thus the more clearly delineated as a preliminary technique in process of development, aimed at personality evaluation through selective memory for unambiguous and clearly presented

[1] Presented at the annual meeting of the American Psychological Association, Sept. 1, 1957.

contentual or thematic stimuli. Used as a content, rather than structural, projective technique, it thus derives from, and extends into the auditory modality, the pioneering work on the TAT of Murray and associates at Harvard, with whom Davids collaborated in research antecedent to the present article (see: Davids, A., and Murray, H. A., "Preliminary appraisal of an auditory projective technique for studying personality and cognition," *Amer. J. Orthopsychiat.*, 1955, 25, 543-554).

Davids' auditory projective technique, aptly named the Azzageddi Test after Herman Melville's devil, who confounded and confused those whom he entered, is a cleverly planned and skillfully constructed combination of sentences and phrases put together in the order of a Latin Square design with controlled randomization. The technique was designed to measure selective perception, interpretation, and retention of auditory stimuli representative of eight "dispositions" of personality: sociocentricity, trust, optimism, egocentricity, distrust, pessimism, anxiety, and resentment. The latter five the author combines on the basis of his earlier work into the subjects' responses to negative ideas, which combination he terms "alienation" syndrome. He then correlates these five as a unitary syndrome, with his other measures. The stimulus material of the Azzageddi Test, then, consists of eight spoken passages (the technical details of the presentation itself not described for the reader), constructed by intermingling sentences and phrases expressive of these eight dispositions. Each passage focuses upon and emphasizes one of the eight, but includes phrases and sentences selected to express ideas of the other seven as well. To describe the procedure in more detail, each passage includes four statements reflective of a single disposition and six short phrases each again associated with a different disposition. By systematically rotating the positions of the phrases and statements in order to control for serial position with a view to equating the effects of primacy and recency on the subject's learning, retention, and recall, the author has constructed an auditory projective technique noteworthy both in conception and in design.

An example may be of interest. The major theme of the following passage let us leave for the moment to the listener:

> "Beware of sly men, laden with malice, breeders of dirty lies, and smearers of character. Boiling with rage. Opportunities for happiness are unlimited. Disbelieve. It is not what we take up for ourselves, but what we give up for others that truly makes us rich. Trustworthy men. Have confidence in yourself and in your future. Life is a great goblet of glorious posibilities, brimming over with enough delight to make us giddy. Equality for all. Never lose faith in your fellow men. Desire for power. Let neither sorrow nor disappointment bend you down to earth. Peril upon peril. People love themselves above all others."

It might seem fairly obvious that the major theme is optimism, with the various dispositions of the alienation syndrome only jarring but not overweening. However, in testing out the instrument with normal Harvard College students, being studied in a long-term and large-scale personality study descended in procedures and techniques from the earlier *"Explorations in Personality,"* quite varied responses were obtained. These responses in turn correlated well with other test measures and with ratings based upon independent interviews and with analyses of the students' autobiographies. Selective perception was clearly demonstrated, and the potentials of the technique for clinical use were pointed out by Davids, including prediction in a replication, with suitable statistics, including non-parametric statistics where applicable, and with appropriate scientific caution.

In addition, as criterion measures of past experience and of present personality dispositions the author constructed two inventories, an "inven-

tory of life experiences," and an "affect questionnaire." Although these two instruments were not subjected to rigorous standardization, inasmuch as they were constructed as experimental tools for the purpose of the present research, they were planfully and skillfully produced. A thorough presentation of the affect questionnaire is given in Davids' article, "Alienation, social apperception, and ego structure," published in the *Journal of Consulting Psychology*.

Davids' original and novel projective technique was devised for and employed in the pursuit of testing hypotheses derived from current learning theory, a usage of the projective point of view with which, I am sure, we are all in accord. Projective techniques and methodology, so used, enter properly into the larger science of psychology, where they belong—a far cry from the days when projectives were new, practitioners with projectives few, and accusations of cultism were often made about them. Davids seeks to test, through his technique, which properly fits into projective methodology, the broad fact of the acceptance in all current theories of personality, based, Davids notes, on the abundance of evidence from psychoanalytic case studies and from experiments in conditioning and learning, of the influence of experiences upon the course of development. More specifically, in the field of perception-personality relationships, he is influenced by, and has planned his continuing research in line with, the Bruner and Postman "expectancy" or "hypothesis" theory of perception, "hypothesis" being defined as a highly generalized state within the individual to respond selectively to certain classes of events in his, the perceiver's, environment. Past experiences and confirmations, and present personality dispositions become determinants of what is selected, what is retained and readied for recall, and what is not. Viewed as dispositions more than as structure, this readiness might perhaps be better explained by the more fluid transactional theory of perception, in which the object-*as*-perceived is altered by the perceiver in the act of perception, rather than in interactional theory of interaction between presently fixed perceiver and fixed object, which Davids selects. Projection is seen by Bellak (in Abt and Bellak, *Projective Psychology)* as the greatest degree of apperceptive distortion, the latter defined as the new experience being assimilated to and transformed by the apperceptive mass, the residuum of past experience. Projection thus becomes only an extreme form of alteration of the stimulus itself, that is to say, it is itself altered in the transaction. Selective perception may enter into the selection of theory. However, this issue is not of primary importance here. Regardless of the perceptual theory, it is agreed, and Davids operates upon the assumption that dynamic factors influence and determine perception.

Davids' research, in particular, brings projective methodology into the field of experimental studies of selective perception as an ego defense against anxiety, studies beginning with the earlier work of Postman and Bruner, and continuing with the more recent work of these experimenters, and of McGinnies, Lazarus, and others. To these studies in selective perception and attention might be added such concepts as the "selective *in*attention" of the neo-Freudian, Harry Stack Sullivan. Within this interactional theory of perception, Davids employs a phenomenological point of view. The stress is upon the "internal" factors in perception, rather than external factors, for which, in agreement with Abt and Bellak in their *Projective Psychology,* the evidence is prepotent. Need and value, i.e., dynamic and motivational factors, assume primacy as perceptual determinants. The perceiver is "set" by his past experience and present dispositions to respond in selected ways. And as with the TAT, Davids "sets" his college

student subjects by an appeal to their intelligence. The extent of their involvement and the highly varied and personal results obtained are eloquent testimonials to the prepotency of such an appeal. Parenthetically, it might be added that comparable "sets" on the Rorschach have been shown in studies by Hutt and Gibby and by the present reviewer to have comparable effects upon Rorschach protocols — and to suggest, incidentally, the need for carefully standardized presentations of projective test instructions in clinical practice.

Davids' research is conceptually related to phenomenology, with perception defined in terms of the world-as-experienced and thus to self-psychology. The internal factors in perception are stressed, as related to the subject's internal frame of reference and, more specifically, to his largely conscious self-concept. Davids asks his subjects to rate themselves on past experience and on present affective personality dispositions, and to rate others similarly. He found the greatest statistical support for his third hypothesis, that those subjects who were themselves higher on the alienation syndrome than were their apperceptions of the degree of alienation of the "average Harvard student" selectively perceived and recalled a greater proportion of auditory stimuli representative of alienation than did those subjects who were lower on alienation than their apperceptions of the "average Harvard student." Here Davids' study may be criticized on the grounds that the subjects' self-ratings and their ratings of others are primarily cognitive measures and do not adequately take into account the fact that affectively determined selective distortions may equally affect the responses to the "inventory of life experiences," as well as the "affect questionnaire" as the subject rates himself on it, his "ideal person" sort (which is still a *self* sort, witness the theory and research on the concept of "ego-ideal"), and his projections (from himself) as to how the average student would rate the items. All of these ratings tend to be self-ratings and thus are subject to the distortions stemming from the personality characteristics and dispositions of the subjects. The so-called indirect measure of ratings attributed to the "average" student only reflects the more strongly the subject's feeling of alienation from the others. In assuming that the ratings are valid criteria, the author negates his own assumptions that selective perception, retention, and recall are determinants of response. Repression and suppression are just as likely on the questionnaires as on the auditory projective Azzageddi Test, unless we make the assumption, which we will not make, that the so-called objective personality measures are immune to these effects. Davids does correlate his findings with other findings on the same subjects. The study, however, would have been enhanced if the subjects had been evaluated for the same eight factors on other measures. More valid external criterion measures might then have allowed the author to state more definitively that "hostile, unhappy, etc., persons" tends to respond thus and so on a technique designed to evaluate selective auditory memory, rather than all he can really say, that persons who tend to see, and to note and to recall, hostility, unhappiness, etc., in themselves, and in others, also tend to hear it. In clinical practice we know that persons who distort reality most conspicuously are often those who report themselves as having no problems, that careful and cautious paranoids, for example, may not admit to having feelings of hostility and yet be seen as extremely hostile by others and tend to perceive hostility in their environments, etc. More applicable external measures, or at least a fuller reporting of comparable scores from the measures the author used, might allow for the presentation of "discrepancy scores" between the self-ratings and the criterion measures on the dispositions being studied. It would also

have been of interest to compare the accuracy of the recall and the kinds of distortions made by those subjects rated on the various measures as "alienated," and those rated as not.

To summarize briefly, Davids' research was selected for commendation, as noteworthy research in the field of projective techniques, on the following grounds: its originality in conception, its application of projective methodology in a relatively neglected but highly important sense modality, its application of projective methodology in the testing of theory and relating findings back to theory, together with its excellence and sophistication of research design and research execution. We sincerely hope that Davids will continue in this line of research.

Reference

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Symposium: Research In Projective Techniques[1]

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The research study published during the period under consideration which impressed me most is entitled: "The Negation TAT; a projective method for eliciting repressed thought content" by Richard M. Jones, Harvard Psychological Clinic. The following is a brief summary of this study. The subjects were asked to tell two stories to each of five TAT cards, first under standard conditions and then with the instructions to make up the most unlikely story that came to their minds. For each subject the five pairs of stories were given to the subjects' psychoanalytically oriented therapists with the following instruction: "Which story of each pair does more lucidly suggest this patient's repressed psychic content, as you know it?" The therapists did not know by what method the "unlikely" stories had been obtained and any concrete allusions to the negative instructions were deleted. The results indicate that in 46 out of 55 pairs of stories the negation story was rated more suggestive of repressed psychic content. This differential is significant beyond the .001 level. The idea of the negation TAT is based on the Freudian concept of negation as a mechanism by which repressed images can make their way into consciousness. The following is an example of one patient's negation story to Card 8BM.

"He's going to have a baby. Maybe we go back to Christmas at Devil Island when the convicts would hide these tubes, or vials, or whatever you want to call them, up their rectum. They kept their valuables in them. But then again he's not on his back, on his stomach. The doctors could be, say, mercenary, and they are trying to take out his money. He had a money belt sewed on his stomach."

This example and others make us wonder whether the judges rated the negation stories as more suggestive of repressed content because they are more irrational and "crazy" and less close to the visible stimulus, rather than because of their specific content. Jones sees this difficulty and suggests further studies in which the Negation TAT should be tested as to its efficacy in distinguishing between individuals on the basis of specific repressed material.

Jones considers his study an exploratory one, and he suggests that the utility of the Negation TAT for diagnostic purposes should be further explored.

After having summarized the study of my choice, I ought to say why it is "better" than any other study published during the same year in the journals which I covered. While trying to do that, I discover I might be in the embarrassing position of engaging in an enterprise which goes against my philosophy. I believe we know so little about psychological research, about the methods to be used and the findings to be sought that we should be extremely careful about setting standards for "good" research. If we do so, we might easily strengthen preconceived notions of what research should be like and suffocate innovations which are so badly needed. There will always be some eager beavers who will try to imitate the research certified as "good" by the SPT. Research which might be bad according to traditional academic standards might prove extremely stimulating to the field in an indirect manner.

Thus, I have found many studies

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for the period and the journals which I covered that can be predicted to boost the development of psychology. These studies are qualitatively so different, i.e. they are good in so many different ways that it would be as impossible to say which one is best, as it is impossible to say whether a fur coat is better than an automobile. From these studies I have selected one which has special personal appeal to me, because it has characteristics which I value highly on the basis of my own subjective value system. These characteristics are:

(1) *Originality.* Jone's study introduces a principle, namely the principle of Negation, into the field of projective techniques.

(2) *Holistic methods of measurement.* Respectable academic psychologists might object to having a study singled out which uses only 11 subjects. However, I feel the small number of subjects is compensated by the type of information available on them, namely the judgment of their therapists. In my opinion such ratings compare favorably in validity to objective test data and to similar information.

(3) *Applicability.* If the Negation TAT turns out to be what it promises, it would fulfill the need for a technique which penetrates defenses as effectively as the Rorschach, and which, at the same time, produces concrete case history material as does the TAT. It might become a source of information on how life experiences, such as human relationships, are assimilated in deeper layers of the personality.

(4) *Generality rather than specificity.* The scene is likely to change in the field of psychology within the next decades. Maybe such tools as the Rorschach or the TAT will be discarded as diagnostic techniques and only taught in courses on the history of psychology. Therefore, research in projection which goes beyond specific techniques seems of more lasting significance than more specialized studies. The Negation technique can be applied to a variety of diagnostic situations and, as Jones writes, to the exploration of primary processes in general. Asking people "what is not" might be an avenue to their unconscious. Thus, this casual little study using 11 subjects might go a long way since it introduces a basic idea.

This concludes my remarks on Dr. Jones' paper. Dr. Feifel has generously given me a little more time, and I should like to use it to discuss a special problem regarding projective techniques which should be given more consideration in clinical practice and in research, namely the problem of "Diagnosis for what?" I dislike the tendency of using tools and techniques for their own sake. Maslow has called this tendency Mean-Centering as contrasted to Problem-Centering and has discussed its disadvantages in his book on *Motivation and Personality.*

I am afraid that our diagnostic tools are often used in an overly mean-centered fashion. For example, quite frequently a battery of diagnostic tests is given to every patient entering psychotherapy. I ask: how are the results of the diagnosis used during therapy? How much do we know about modifying the therapeutic process in accordance with the findings from diagnostic techniques? I do not imply that we do not have any knowledge of this kind, but it strikes me as somewhat scanty. I have found the Rorschach very useful when trying to decide whether a person is border-line psychotic, and this is good to know because interpretive therapy may precipitate a psychosis, and strengthening of neurotic defenses might be indicated. I could probably give many more illustrations of this kind. But I still believe that our knowledge of what to do with diagnostic data is very poor as compared to our knowledge how to produce such data and to our eagerness to do so.

This criticism is not limited to the applicability of diagnostic data to therapy, but to any recommendations

to be made or to any clinical procedure to be applied on the basis of projective diagnosis.

The above does not touch the use of projective techniques for the sake of advancing our knowledge about people. Such advancement is a worthy end in itself, and it is likely that the applied clinical aspect of our field will eventually gain from it. Thus, giving batteries of projective tests in public clinics might be defensible purely from this point of view. It is less easy to defend the practice of many private psychiatrists, for example, who refer *every* patient to a psychologist for expensive psychological testing. This is sometimes useful, but often it is not, and I have the feeling that it is occasionally done to impress the patient with fashionable gadgets. I would hate to see our profession lend itself to such commercialism. I am saying this at the risk of putting my foot into someone's bread and butter.

Concluding, I should like to make the following positive suggestions: Let us gather all the available knowledge on the relationship between personality variables in the patient and clinical procedures to be recommended, and let us search for more knowledge of this kind. Then, let us concentrate on the attempt to diagnose these variables, not necessary to the exclusion of others. Let us be guided by this principle when we develop projective tests, when we interpret them, and when we do research on their validity.

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Research with Projective Techniques[1]

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Current psychology and psychiatry now recognize that every person has mental devices for translating his sensory and perceptual impulses in such a way as to aid and bolster his equilibrium and self esteem, or to build his fears and anxieties. We all have our ways of viewing, interpreting and dealing with the events of our world.

That psychologists who *know* that they are always liable to such distorting are *still* willing to try to do research and to strive for such an unattainable goal as objective truth, is a measure of the strangeness of our motivation as well as of the range of adaptability and capacity for self deception, perhaps, of which man is capable. However, the fact that researchers do put in great effort, have strong motivation, are honest in intent, and try to be objective, does not in itself assure that the research work will be good or accurate. So it is that there is a Society for Projective Techniques, that some of its members do research, that it has a Research Committee, and that here we are!!

Projective methods are based upon the assumption that these shadowy non-logical, motivationally determined ways of valuing and perceiving and acting, can sometimes be apprehended and inspected. The individual in the testing situation acts in accordance with the social climate which is his peculiar, unique own; and if he has had no experience with other kinds of climate, he may easily assume his projected world is naturally the same for everyone.

We look to projective tests to provide assistance in two directions: 1) such tests may aid the clinical practitioner in the appraisal and treatment of any individual. Thus, as they fit into practical affairs, they require transition and interpretation that is interdisciplinary; and that has practical consequences in that they can affect the course of treatment, the life and happiness of troubled persons. 2) Studies with these tests may bear upon the theory of personality and of mental illness, and if so our scientific understanding and basic orientation for treatment are forwarded.

It is in this context that the Research Committee of the Society for Projective Techniques has arrogated to itself the task of selecting a few research publications of the year 1956, which fit in with *our* preconceptions of what is good research. We hope that this selection compliments the authors, and that it encourages them and others to pursue our common fantasy research goal.

Since our committee has not this year decided the criteria for the selection of the research articles, each of us in our present reporting relies upon his own clinical intuition and personal judgment in evaluating a particular publication chosen by the Committee and assigned to him for report here. Therefore, "the statements made do not necessarily reflect the opinions of the Committee, the Society, the Hotel, etcetera."

I am happy with the choice of the committee in assigning me a very neat paper by Kenneth Purcell on "The TAT and antisocial behavior," which appeared in the December 1956 issue of the *Journal of Consulting Psychology*.

Purcell's research consists in an analysis of some of the TAT responses of a group of army trainees as related to their case-history record of antisocial behavior. The study seeks to

[1] Presented at the annual meeting of the American Psychological Association, Sept. 1, 1957.

establish the relationship between selected aspects of TAT reports as representing covert fantasy material, used to predict some aspects of overt behavior described as antisocial.

Previous investigators, such as Henry Murray and Nevitt Sanford failed to find a substantial direct relationship between the intensity of fantasy aggression and its over-expression in a middle class population. Others have reported such a relationship with lower class boys.

Purcell had available at the Mental Hygiene Clinic at Fort Dix army trainees who include both middle and working class persons, and some persons whose behavior has been pretty antisocial.

The measure of overt antisocial aggressiveness was based on the soldier's social history, and was rated on a 7 point scale. The most aggressive person, for instance, had this kind of a record: many truancies, delinquencies and class room misbehavior; in a reform school for four years; convicted of armed robbery; had assaulted several people; was A W O L eight times; caught in possession of drugs; had 4 courts martial; now facing court martial for wrecking the stockade dispensary.

To predict the amount of overt antisocial behavior, Purcell looked to the analysis of the TAT stories from 4 points of view, for four scoring systems. The separate scores were not simply empirical, but relate to the theory he had about personality. He viewed the antisocial behavior as a resultant of a disequilibrium between an impulse system and a control system (as in Freud's id and superego).

1. The impulse system in regard to antisocial behavior was to be measured by a count of instances of aggression in the TAT stories, such as fighting, criminal assault, getting angry, criticizing, running away, resisting coercion, being negativistic, lying, stealing, cheating, dominating or restraining someone else, and so on. The control system — which may or may not manage to control the impulse system—is viewed as two-fold; fantasy punishment that is either external or internal. 2. The external punishment score counted the individual's references to external consequences, being caught and punished. It was obtained by summing the frequency of themes directed toward the hero of assault, injury, threat, quarreling, deprivation, domination, physical handicap, or rejection. 3. The inner control system summed the themes of suicide, self-depreciation, feelings of guilt, shame or remorse, and — with qualifications for the actions of the hero—injury to loved ones. 4. A remoteness score was developed on a 7 point scale, to differentiate direct aggression, in fantasy, from more remote aggression such as displacing the object from a person to a group or society, or an animal or an inanimate object; or to shift the action out of the present to some more distant place or remote time. The reliabilities of these rating scales is reported to be between .81 and .96, and intercorrelations between them are substantial.

At this point it is worth noting that the notion of antisocial behavior has taken on a different shade of meaning, being identified or specified as "*aggressive* antisocial behavior," and it is aggressive fantasies in the TAT stories that are to be counted to predict the antisocial behavior.

Now to summarize the research program in terms of Purcell's hypotheses. Persons have many impulses toward aggressive antisocial action which can be identified and measured in TAT stories; and some of these impulses can be found carried out in overt antisocial behavior. But the impulse to action may be checked by threats of punishment, some of which may be external like being caught and punished, and some may be inner controls such as remorse and guilt. The hypotheses to be tested then, are

> that the amount of aggressive behavior is directly related to the num-

ber of TAT aggressive thoughts;

that with the more antisocial individuals the aggressive fantasy in TAT stories will be more crude and direct; that overt aggressive activity will be inhibited by both external and internal controls, and the inner controls (i.e. remorse) will be more inhibitory than the external controls (i.e. fear of being punished);

that some individuals with strong antisocial trends may utilize themes of external punishment as a justification for more counteraggression, rather than as inhibitory to his own aggressive activity.

With this program the research proceeds: 57 trainees who were psychiatric referrals, not psychotic, not brain damaged, were used as subjects. They were divided into three groups on the basis of their antisocial behavior scores. Comparisons were made between the groups, using both analysis of variance, and "t ratios" of group differences, supporting all the hypotheses. Why Purcell did not directly correlate the antisocial behavior scores with the various scores on the TAT material I find hard to understand.

Before presenting some of the results I wish to comment on the use of a word or two, and discuss a topic of some theoretical importance.

The comment has to do with a shift in the author's use of words and hence meanings, in describing TAT fantasy aggressions. He first views these as part of an impulse system; but then he talks about fantasy aggressive *needs*: "the gratification of antisocial needs," and, again, "the correlation between covert needs (that is, fantasy) and overt behavior."

On what basis is there an equation between fantasy, impulse and need? How can fantasy be used as identical with need?

Now I want to say some things about Purcell's concept of aggression. Somewhere I get the feeling that, to Purcell in this paper, all aggressive behavior is assumed to be antisocial, and to be bad. This may be an error of interpretation on my part, but it is based upon some confusions in Purcell's development of his problem.

The title of the research is "The TAT and antisocial behavior." Now antisocial behavior is clearly a term of social functioning, not a scientific term, and hence very difficult to define or describe behaviorally. Purcell goes along with Redl and Wineman's account of the "delinquent ego" which is defined as "the ego whose every effort is bent toward obtaining guilt-free and anxiety-free enjoyment of delinquent impulsivity." This seems not only a circular word-game, where you define the delinquent ego in terms of the delinquent impulsivity, and vise versa, but also leaves me at a loss regarding either term. Antisocial behavior and delinquency have meaning enough in the context of the law, as regards obedience and disobedience, flouting the edicts of authority figures; in terms of overt defiance of the rules and directives of officials.

Purcell makes no attempt to define these terms. He takes an easy way out by simply using social evaluations to classify his subjects. He lets the evaluation of the life history of his 57 subjects, in terms of their conflicts with authority, serve to establish their position on his scale of antisocial behavior, and thus to provide the criterion ratings.

He cannot avoid discussion; and at one point he seems to assume that antisocial behavior is identical with aggressive behavior. He says he is studying "certain personality variables, particularly in terms of the impulse-control balance, in order to establish their relationship to over-aggressive tendencies." That is, to the criterion scores of antisocial behavior. Of course a person might express antisocial behavior by complete withdrawal from society, or in many other ways. But here, it means aggression that is hostile.

Overt aggressive behavior may not be legally antisocial even though it may still be destructive. For instance many men in the armed services dutifully carried out their assignments, and sometimes found great satisfaction in hitting the target. Members of the fire department and the police department often engage in overt aggressive behavior, but it is not usually thought of as antisocial. The behavior of the athlete, of assertive scholars, of vigorous salesmen, of young men seeking to fall in love, may be overt aggressive behavior, but it is often far from being always antisocial, delinquent, or bad. Purcell has not considered or allowed for these possibilities.

On the other hand, in dealing with the TAT stories, Purcell objectively takes the count of various kinds of aggressive behavior, in fantasy, but does not distinguish between overt and covert fantasy acts. In the schedule for selecting aggressive actions, no attempt is made to sort out, for instance, fighting and getting angry, from resisting coercion or rejecting someone.

In my own practice I find many people who are inhibited and fearful of rejection—whose "inner control system" goes into operation so strongly that the therapeutic task for a while is to free them so they can permit themselves some kind of assertive expression. There are of course the persons in the acting-out direction, who likewise mis-perceive reality in their own way, and act impulsively on the basis of a limited segment of a situation.

Purcell, however, has set up a one dimensional scale for his criterion of antisocial behavior, as aggressive. I feel it carries with it the implication of all aggressive action as being hostile, bad impulses, which should be checked by threats of punishment or of shame and guilt. See p. 455, of Purcell's paper for "the deterrent function" of "anticipation of punishment as a consequence of aggressive behavior."

Let us now return to the presentation of Purcell's results. We have seen that the data strongly support all the hypotheses; and additional speculation and manipulation of the data bring out other interesting items some of which I shall briefly mention.

1. A positive relation was found between TAT fantasied aggression and aggressive antisocial behavior; and this relation held true for both classes when the subject population was divided into middle and working class. Earlier studies had failed to show this relationship, but Purcell points out that his subjects showed a tremendous range of aggressive antisocial behavior; and he found evidence to support the notion that in certain impulsive antisocial individuals, fantasy is no substitute for direct action but actually is a spur to act-out.

2. Purcell did find that lower-class members show greater antisocial behavior than middle class members, and he accepts the notion that this reflects a freer rein to impulse in the lower class. But the results here reported still show that whether in middle or lower class men, strong antisocial behavior goes along with more aggressive fantasies and/or weaker internal control systems.

3. Regarding remoteness of aggressive fantasy, the antisocial subjects were more direct and less remote than the nonantisocial. Thus, the defensive inhibitions alter not only the quantity of aggressive themes, but also the way they are formulated.

4. Anticipation of external punishment as a consequence of aggressive behavior occurred equally with the various social and antisocial groups. But among the least antisocial group there was a significantly larger number of punishment themes unrelated to aggressive acts, as if punishment may be just arbitrary.

5. The antisocial subjects tend to

justify their aggressive reactions by pointing to harsh treatment. The antisocial individual seeks proof that the world is against him and hence he has no need to recognize obligations, but rather has a right to get even. Purcell suggests, then, that a policy of retaliation would tend to reinforce this maneuver and make it easier for the delinquent to pursue his antisocial course. The evasion of guilt, shame and remorse is more prominent in the antisocial individual than is the lack of awareness of meaning of the action or concern with punishment.

6. Finally there is the hypothesis that fantasy behavior has a compensatory function—that some people act aggressively, others take out the impulse in fantasy. Purcell's findings support not this hypothesis, but the opposite view: that fantasy aggression is positively related to overt aggressive actions. Purcell, however, does not find this a contradiction. He suggests two notions that need to be considered: it may be that his subjects included some persons characterized by such extensive antisocial behavior and aggressive fantasies as to obscure any shifts in the nonantisocial subjects. And further, he suggests that people may differ as to the uses of fantasy aggression: for instance that among the antisocial individuals aggressive fantasy may serve as inciting and a spur to antisocial behavior; while among others, such as his nonantisocial individuals, such fantasy may serve more as an adaptive and defensive measure, a substitute.

This completes the presentation of the findings of Purcell's study. As he points out regarding the uses of TAT material, it is important to evaluate the inhibitory, repressive, controlling items as well as to score for the usual Needs and Presses.

In conclusion, may I commend Dr. Purcell for the insightful selection of the material available to him as a basis for testing these notions about fantasy and overt aggressive behavior, for the thoughtful analysis of TAT materials, and for his effective utilization of statistical and analytical procedures to provide clear-cut results.

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Europe — Once Over Lightly[1]

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As one of 98 psychologists on the APA charter flight to the XVth International Congress of Psychology, I had a very brief opportunity (less than four weeks) to learn something of current European approaches to research with projective techniques. By modern standards, this probably makes me expert enough to write a book. All I can offer, however, are some light weight, superficial observations, strictly limited by personal experiences that may or may not have been representative.

The APA flight was unique, bringing together on a DC 6 B airliner a total of 98 assorted colleagues, including wives, husbands, mothers, fathers, and one each son, daughter, brother, and sister. Home addresses ranged from Bar Harbor, Maine, to Tacoma, Washington. Sociability was fostered by cramped quarters, and later by an all too brief stop at tax and duty free Shannon.

The Brussels Congress was sponsored by the International Union of Scientific Psychology, a body of national psychological associations currently numbering 26, including the APA and its Soviet Union counterpart. Some 1,200 individuals attended, 30 percent coming from the United States. It may have been symbolic perhaps of the sheer size of American psychology which must at times appear almost overwhelming to our friends abroad.

Our symposium chairman asked me to bring back "the current trend in Europe," and to say something about similarities and differences between European and American approaches. The assignment was an impossible one; at least for me. It all depended on whom you met, what you discussed, where that colleague was working, and, perhaps most important, the interests and attitudes of the senior professor of the institute or department concerned. I found no one single European trend.

If there was any broad thread through the varied papers and symposia, it may have been the considerable emphasis on and interest in American statistical techniques. When a question about Latin squares or factor analysis evoked a puzzled look, it was considered strange that I did not know. German graduate students and instructors were particularly noteworthy for their quantitative sophistication, far more than might have been expected in view of the philosophical psychology of their elders.

The one symposium on projective techniques, ably organized and chaired by Gardner Murphy, featured a French and English language debate on theory, perception, and validation. It was argued by several participants that theoretical matters only confused validation, and that it might be better to forget about theory and concentrate on the practical utility of projective methods. This attitude was also reflected in two paper reading sessions; it was countered only rarely, most effectively by Henry Murray. It is difficult to estimate, however, how representative this was of European projective psychology; very few people well identified with the field were present. The Congress seemed to attract colleagues with more experimental interests.

What impressed me most at Brussels was that European psychologists read our journals, far more than we do theirs or even our own. Many papers

[1] Presented at the annual meeting of the American Psychological Association, Sept. 1, 1957.

indicated that pertinent American research had at least been noted. The Russians were well informed, not only on conditioning, but also on child psychology (which they call pedagogical psychology). Their studies are still prefaced with a bow to Pavlov and there is no official mention of Freud or Rorschach. In later conversations, however, it became clear that tests have been developed and are being used. The reporting of numerical scores is discouraged and, I was told, there is no demand for projective techniques in either the Soviet Union or the satellite countries.

After a few beers with continental colleagues (in the Cité Universitaire and elsewhere) it became quite evident that clinical psychology, as we know it, has almost no counterpart abroad. University psychology departments seldom welcome clinical research and only a few give graduate degrees in the area. In most countries those who consider themselves clinical psychologists are physicians with specialized postgraduate training. They are taught to administer and interpret projective techniques (by psychiatrists and sometimes by psychologists), and usually are the only ones permitted by law to do psychotherapy. In Switzerland psychologists are not officially allowed to attend medical school lectures but are frequently "invited." Any type of therapy requiring less than 20 hours is called "counselling" and may be practiced by psychologists; if it exceeds 20 hours it becomes "therapy" and must be in association with or supervised by a physician. Psychologists who want to practice as clinicians are often employed in public institutions at substandard pay, usually working with difficult children. Research is frequently oriented toward endocrine disorders, of which schizophrenia is considered to be one.

France, at the moment, offers little hope for clinical psychology. No university gives a degree, all testing must be done blindly, and the entire field is strongly controlled by medicine. Major efforts are directed to the translation of American tests. In Germany it depends very much on where you go and whom you meet. The range is from philosophical psychology (Mainz) through projective personality studies (Freiburg) to experimental, statistical approaches (Marburg), with many other institutes along this very rough continuum. Students are expected to be familiar with standard tests. While child, industrial, and forensic applications are emphasized, degrees are given in the clinical specialty. Italian psychology is expanding in the hands of younger men but clinical services are still limited by cultural elements.

There was no opportunity to visit other European countries but I was impressed by the high calibre work presented in Brussels by colleagues from Holland and the Scandinivian lands. The American influence seemed strong, particularly along industrial-clinical lines. Great Britain reflects the differing approaches of Tavistock and Maudsley, with much of which you are already familiar.

After the Congress I asked Ewald Bohm to comment on Europe and America from the standpoint of a European. As you know, he is one of the top Rorschachers abroad; his textbook is a best seller (Huber) with an American translation in press (Grune and Stratton). Bohm talked about "scientific superstitions" and offered the following as particularly descriptive of Americans:

1. Constitution is a hoary concept; heredity was not discovered by Mendel but by the Nazis, and has long since been tossed out the window.

2. Literature more than two years old no longer contains any scientific truths and therefore may be safely disregarded.

3. All scientific progress is "made in USA"; the other 92 percent of the world's inhabitants whose native language is not English are "underprivileged people." Since what they do is of no scientific interest, there is no

sense in learning foreign languages.

4. It is below a Senior Psychologist's dignity to use a test, especially the Rorschach, without introducing technical changes. Changing the ground rules as much as possible affords the inestimable advantage of not having to pay attention to the publications of other authors; anyway, their findings are no longer strictly comparable.

5. Everything and anything can be proven statistically, especially concepts and hypotheses lacking intrinsic quantitative aspects. A few curves and diagrams "prove" the scientific value of any study, regardless of the pertinence of the underlying rationale.

Of course, these are tongue-in-cheek comments, but Bohm feels equally severe about the "scientific superstitions" of his European colleagues. He suggests five:

1. One can philosophize about any problem; it is therefore below the dignity of an academician to present any case study without a philosophical introduction.

2. The epitome of a scientific education is attained when you can express yourself in the most difficult to comprehend manner. The reader develops feelings of inferiority and his admiration for the author grows accordingly.

3. An ingenuous formulation is far more important than the correctness of the assertion made.

4. A scientific theory can be "beautiful" even without a solid foundation of facts; such lack does not impede scientific theory. After all, the Americans will surely evaluate it sooner or later, and they have IBM machines.

5. The historical introduction is more important than your own contribution to the discussion of a current problem. It is therefore best to begin with Adam and Eve, Noah at the latest, and to use as many pages as may be required, at least four times the length of the discussion. Anyhow when your scientific qualifications are judged, only the total number of pages really matters.

While Bohm's comments are meant to be humorous, there is some truth in each. We all have our idiosyncracies, and sometimes it helps to look at them through the other fellow's eyes.

Three weeks later, walking on the Champs Elysees or surveying the scenery of Place Pigalle, there were joyful reunions with APA charter flight colleagues. We were eager to talk of our travels, ranging from Yugoslavia to Israel. The return trip was even more sociable and the whole idea seemed well worth repeating. (The saving in money alone was 45 percent.)

In all, the Congress was a stimulating and satisfying experience. It provided an opportunity to meet people with whom I had corresponded or whose work was similar to mine. There was time for exploration, for discussion, and for argument. Friendships developed spontaneously, with warmth quickly penetrating the usual reserve. It was completely different from the usually hectic, job shopping APA. We are already planning to attend the 1960 Congress, wherever it may be.

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Research with Projective Techniques, Some Trends[1]

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The authors whose provocative papers were selected and the discussants' evaluations of them leave little more to be said about their specific findings. They have made contributions which will eventually prove to have added some knowledge, stimulated new research efforts, and suffered from limitations. They will be viewed as authoritative by some for a number of years and then their defects will catch the eagle eyes of others who from the vantage point of next year's knowledge will go beyond these findings. It is easier to find flaws in research than to rectify them.

What I should like to do is to extrapolate from these three papers to trends in clinical research which they seem to exemplify. From the frame of reference of papers that I have screened, rejected and published for the *Journal of Projective Techniques,* a number of significant trends seem to stand out.

Projective techniques had first to be sold when they appeared on the American scene. Few psychologists were clinicians; more were psychometricians, academicians, or research workers of one sort or another. The kind of individual-oriented psychology that supported projective methods did not find a well-prepared conceptual soil awaiting it, excepting, perhaps, in the phenomenological movement. When the Rorschach Research Exchange was inaugurated 21 years ago, it was essentially the only medium in this country for the interchange of information about projective methods. At present the Journal publishes less than a fourth of U. S. articles in projective methods. It played a somewhat proselytizing role in its earlier years for it expressed the interests of a small segment of the psychology community, those interested in the intensive study of the individual. Hence the orientation of many of its papers was toward demonstration of the clinical value and diagnostic cook-book use of projective methods, particularly the Rorschach. Even as recently as a decade ago, the Journal's articles tended to be diagnostic, normative and didactic and somewhat out of the arena of personality theory per se. The three present papers demonstrate the value of extracting problems from clinical realities and handling them with the best traditions of psychological research methods and theories. It is noteworthy that these papers make use of projective methods less for diagnosis than for exploring the nature of personality. Perhaps the split psyches of those converted during the war years from academicians to clinicians are mending. Or are we developing a new psychological genus whose psyches find no conflict between research methods and clinical interest?

The Journal now publishes very few case studies or blind empirical group difference studies. It has also expanded from the original Rorschach preoccupation to include many tests, some of which are only vaguely projective in nature. Perhaps this is due in part to the gradual inclusion of projective psychology into the body of perceptual-personality theory.

Developmental changes in our theoretical formulations have led to ingenious experimentation upon more complex hypotheses, more germane to the understanding of individuals. Not long ago and now to a lesser extent, clinical research studies were made on the basis of rather simple motivation-

[1] Presented at the annual meeting of the American Psychological Association, Sept. 1, 1957.

al data, drives, traits and the like. Experiments such as those of Purcell, Jones, and Davids show clear recognition of the futility of attempting to predict directly from fantasy or other evidence of single drives to overt behavior. The problem, of course, is that there is a good deal of psychologic substance between drives and behavior. In other words, the expression of a motive in one context or at one level of psychological organization is not highly correlated with its manifestations at another psychological level or in other stimulus contexts. We are piling up more and more evidence that the ego is not a flat surface or a psychometric map, but has hierarchical properties, particularly when the focus of inquiry is the individual person.

Psychologists are tending to shift their emphasis from drive constructs to more complex concepts such as ego defensive and coping mechanisms. Similarly, the vanguards of psychoanalytic theory are moving toward putting the id in its place as a less useful descriptive and explanatory concept than those of ego functions and object relations. The evidence now is that the crucial data are not the motivational content of fantasy alone, but the unique ways in which these universals are handled in interpersonal situations. Our authors suggest that the mere presence or absence or frequency of occurrence of projectively aroused fantasies are insufficient evidence for prediction because they carry no indications of the degree of probability of their expression in specific situations. Jones has found another way of finding out what the hidden motives are. Davids and Purcell reveal some of the enduring internal factors that regulate their expression. Purcell shows nicely how prediction or postdiction is enhanced by studying intrapersonal integrations of aggressive drives and social expectations. Davids demonstrates that an interaction effect or dynamic relationship between experiences and present attitudes improves prediction over biographic data alone.

Part of the de-emphasis of trait concepts has involved a lessening of unidimensional characterizations. We are learning to think of persons not as more or less hostile or passive or zomboid, but as manifesting specific forms of these behaviors in specified kinds of situations.

Additionally, recognition that fantasy motives are not always directly predictive of overt behavior has been coupled with a retreat from earlier clinical beliefs that the projective protocol reveals the whole story. The projective picture of a client is seen more clearly as one of several pictures. Behaviors revealed in sentence completions, TAT stories and Rorschachs may differ radically from one another and from overt behavior.

Test productions emerge in a new light according to an old principle: that behavior is a function of the stimulus situation. Many recent studies have pointed up the unreliability of response to a single projective test as a function of examiner variables and shifting test-taking attitudes on the part of clients. Similarly, more attention is being given to the nature of the stimuli to which the performance seems to be oriented. Many of us have tended to view the person as seen through a projective test as if his test behavior were endogenous, enduring, and complete. While it is of some importance to know how an individual will endogenously structure his life, seek out and selectively react to stimuli, we are becoming more aware of the fallacy of detaching the projective test estimate of endogenous processes from the stimuli.

Psychologists are becoming increasingly inventive in concocting new stimuli of controlled degrees of structuredness and predetermined dimensions of content in which ambiguity, hence opportunity for variant behavior, is present. There is no longer one projective technique. There are, rather, growing numbers of projective

stimuli which have been designed much as aptitude tests are, to answer concrete diagnostic, descriptive or predictive problems. The evidence suggests that the Rorschach is useless for predictions of aviator success. Either the variables sought out in the Rorschach are inappropriate or the Rorschach is not the proper instrument from which to expect such an answer. David's Azzageddi test is significant not only for its clever use of the difficult auditory mode, but especially for its purposive structuring of projective stimuli for specific purposes. Davids is clearly implying that not all of what a man is will be shown in a single test situation, that an ink blot is not a crystal ball. Conversely, he is saying that the existence of some enduring aspects of personality organization is demonstrable only when the psychologist creates a favorable environment for its expression.

In these three papers we see signs of dissolution of conceptual and attitudinal barriers between clinician and research psychologist. The writers show evidence of interest in the individual in all his complexity and point out diagnostic and therapeutic implications, yet produce evidence that adds to our conceptualizations about personality. They are clinical and research papers simultaneously. They show a neat fusion on the one hand of scientific rigor without the rigor mortis of scientific method applied to trivialities and on the other hand a lively interest in the psychology of everyday life without the platitudes into which clinicians can so easily slip.

In this respect, some tribute is due to Freudian psychoanalytic theory as a source of research problems and tentative hypotheses. It may prove to have been a happy accident that American psychologists early in the century veered from the influence of psychoanalytic theory and even from the kinds of problems with which it dealt. Proper scientific evaluation of psychoanalytic precepts cannot easily be achieved by the methods of psychoanalysis itself. It might be argued that early indoctrination into psychoanalytic theory may develop a trained incapacity that could interfere with a psychologist's ability to entertain other notions or to examine psychoanalytic notions critically. There is some evidence that intellectual organization *is* affected by one's exposure to the operations, methods and values of one's vocation. It is well that historically psychoanalysis and psychology went their own ways with only the most cool and infrequent contacts. Now that research psychologists have turned their attention to the formulations of psychoanalysis and the psychological phenomena which have been their province, psychologists have the advantages of variety in theory and data and a tradition of scientific method. As recent research publications indicate, psychologists have found in psychoanalytic data a number of provocative ideas which they are able to utilize in their research activities. We seem now to be in a period of evaluating what psychoanalysis has to offer, testing it, adapting it, refashioning it, and integrating it into our own conceptual framework. The number of clinical research papers which implicitly or explicitly are testing psychoanalytic concepts has grown enormously in the last ten years.

Our research discipline is of unquestioned importance in enabling us to tease from a recalcitrant world of phenomena, the anchors in reality that our theories require. There are times, however, when our healthy skepticism may become defensive blindness. There may also be lost time between the emergence of a creative idea and its fruition in demonstrable research data. I am pleased to see that our research trio have an urge to speculate beyond their data, even though it is not yet as fashionable as I should like to see it. The breakthroughs of scientific psychology are likely to come not through cautious

empiricism, but through scientific fantasy tested rigorously. The *Journal of Projective Techniques* is experimenting with a column of indeterminate length as an agora for the communication of ideas, hunches, and fantasies whose authors have neither the time nor the inclination to carry them into the laboratory. Our hope is that the daring and the cautious may be brought together more quickly to their mutual satisfaction.

A few more comments.

Is it chance alone that two of the three papers used the TAT and that the third is an auditory extrapolation of the TAT? Is it at all significant that the Rorschach is conspicuously absent? Perhaps the Rorschach is a less adaptable instrument than thematic material for personality research. It would be interesting to do research on research to learn whether and how the two instruments differ in their research uses. Is it another possibility that the Rorschach having more or less started the trend to projective psychology has primarily a historic status that is being challenged by other methods to the point of being replaced? Or is it finding its proper place as a limited tool having specified utilities? Certainly the spate of papers which attempted to use the Rorschach for everything under the sun has subsided.

Finally: for many of us it will be a gratifying day that sees a symposium similar to this in which we are able to experimentalize the psychotherapy process with such ingenuity as has been shown in the three papers discussed today.

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A Note on Persistent Responses In Longitudinal Rorschach Protocols

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The author has administered the Rorschach Test to a single male subject for the past six years (2). During this period, between the youngster's fourth and tenth birthdates, a total of eighteen protocols have been accumulated.[1] A review of these records discloses three engrams which have appeared consistently throughout the series. While other percepts have been repeated in a scattered pattern, the persistence of these cannot be overlooked.

From rather intimate knowledge of the subject's life history, there does not seem to be an immediate relationship between the child's current life situation and the nature of these percepts. The speed with which these engrams are verbalized upon presentation of the appropriate plates (II and VII) gives the impression that the youngster feels compelled to report them. There are no overt signs of hesitancy to suggest that the subject is trying to recall previous responses to the particular blot-areas.

The first one, elicited at 4 years, is located in Plate VII, D11 (3, p. 221) and shows the following course:

4 years, 2 months– (Second response to the plate) "House, church down here, see there."

4 years, 3 months–2. "A church house far down there." [Inquiry, "Churches have these (steeples) on. If it isn't a church, what is it?]

4 years, 6 months–2. "Church down here." ["Has this steeple, and two windows. L.1. a Catholic Church like Miss Claire (former nurse) used to go to."]

4 years, 9 months–1. (First response) "A church right down . . . " ("Right down this road.")

5 years–2. " . . . and a church down here and the sky covering the church." ["Because churches have these (steeples) and there's a window. That's not all of the church, the sky is covering it."]

5 years, 3 months–2. "Church down here, trees too." ["Two big things (steeples), here's a road, and two trees."]

5 years, 6 months–2. "Church." ("A church has things sticking up and windows.")

5 years, 9 months–2. "A church way back, one hundred miles from there." ("A Protestant Church, I mean a Jewish people's church.")

6 years–2. "A church way back."

6 years, 3 months–2. "A church way down"

6 years, 6 months–2. " . . . and a church."

6 years, 9 months–2. "Church." ("A church, windows and a cross on the very top.")

7 years–1. (Now the first response to plate VII) "A church."

7 years, 6 months–1. "A church way down here.")

8 years–1. "A church, a white church."

8 years, 6 months–2. (Back to being the second response) "A church."

9 years–1. 2. "A church at the end."

10 years–1. "Far away I see a church." (This thing sticking up and two windows. It's far away and it's little.")

This percept has shown little change over the past six years. The author, who is the subject's father, and his wife could not recall any undue event centered about a "church" concept that might have occurred about the time of the child's fourth birthday. While there are churches in the vicinity of the home, there has been no emphasis on them beyond the casual conversation between husband and wife and the usual pointing out of places to a growing and curious offspring. This latter notion is as far as the author is willing to go to account for the persistence of this particular percept. A psychoanalytically inclined colleague suggested, at the time of the initial appearance of this engram, that the youngster must have had an indelibly impressive expe-

[1] Quarterly administrations between 4 years and 7 years yielded 13 records, semiannual testing from 7 years, 6 months to 9 years resulted in 4, and the first annual Rorschach at 10 years added the final protocol.

rience revolving around the sanctity of motherhood and a desire to return to the security of the womb. All this symbolized by the acceptable church concept somehow derived from the popular, albeit socially unacceptable, sex response-area (5) in the "Mother" (4) card, Plate VII. This is imparsimonious speculation and *ad hoc* reasoning. It *is* barely possible that the association is one of congruence of shapes, viz., the stereotyped church design (which incidentally fits the overall form of most local churches) and the shape of the blot area involved in this percept. Why this has persisted is still unexplained on other grounds.

The second (and later the third) engram is located in Plate II. First is the use of the entire plate (W) and then this is separated into D2 and D1+D1. This is the development:

4 years, 6 months—1, W. "Lightning and smoke coming out. I'll show you the lightning (D2) and here (D1+D1) is the smoke, it's black."

4 years, 9 months—1, W. "Lightning and thunder together." ("Here's thunder blasting and here's lightning. Looks like dynamite fell on it; lightning because it's red. Because it's going up in the air like a rocket. Thunder is black and God makes it look like an elephant, but it isn't an elephant. It's thunder.")

5 years—1, D2. "Lightning."

2, D1+D1. "Elephants."

5 years, 3 months—1, W. "Lightning and thunder, two clouds hit together." ("Lightning because it's red. Thunder because it's black and it looks like two clouds hit together, already hit together.")

5 years, 6 months—1, D2. "Lightning."

2, D1+D1. "Two elephants."

5 years, 9 months—1, D2. "Lightning."

2, W. "Two elephants strucked by lightning and kept together."

6 years—1, D2. "Lightning."

2, D1+D1. "Two elephants."

6 years, 3 months—1, D2. "Lightning . . ."

2, D1+D1. " . . . and two elephants pressing together."

6 years, 6 months—1, D2. "Lightning."

2, D1+D1. "Elephants squeezing together."

6 years, 9 months—exactly the same as above.

7 years—1, D2. "Lightning . . ."

2, D1+D1. " . . . and two elephants." ("The black things are elephants. They got their trunks, noses together. They're alive, looks like they're walking.")

7 years, 6 months—1, D2. "Lightning."

2, D1+D1. "Two elephants." ("The black things are elephants. They got their trunks and noses together. They're alive, looks like they're walking.")

7 years, 6 months—1, W. "I see lightning and thunder."

2, D1+D1. "The thunder is shaped like elephants."

8 years—1, W. "Lightning and thunder." ("The black is two clouds hitting together; that makes thunder, and the orange is lightning.")

8 years, 6 months—1, D2. "I see lightning."

2, D1+D1. "Two elephants."

9 years—1, D2. "I see lightning on top."

2, D1+D1. "Two elephants on the side putting their trunks together."

10 years—1, D2. "Lightning." ("It looks like it's exploding, red. Lightning is not red, but in the comic strips, books, lightning is red.")

2, D1+D1. "Two elephants crashing together." ["Got their noses together; not exactly crashing. They are stuffed because elephants are not so zigzaggy (edge indentations). Sticking their tongues out—made that way."]

In these persistent and consistent engrams a more revealing development is observed. Whereas in the "church" percepts the content remains fairly constant and confined to the same blot area (VII-D11), in the second series two distinct percepts (D2 and D1+D1) emerge from the whole of Plate II.

The first "lightning" response to all of Plate II at 4 years, 6 months has its probable origin in the child's vivid experience of an unusually severe hurricane two days prior to testing. The brilliant lightning bolts and thunderous thunderclaps startlingly lit up the house and shook it to its foundations. The four and one-half year old was visibly and frighteningly impressed despite the reassuring presence of his parents. The severe storm flooded the grounds and seeped into

the house. Electric service was interrupted for days—in all an impressive event. It is plausible to infer that this response epitomizes the child's anxiety in stressful circumstances. Three months later the percept still appears but it is embellished by his interest in a popular television program centered about Space Cadets, rockets, blast-offs, and blast guns. Note the first appearance of the "elephant" concept to D1+D1 in the Inquiry. This represents a mitigation of the anxiety associated with the original "lightning" percept.

Prior to the 5 year, 3 month test the young boy was given an explanation of the phenomenon of a tropical lightning and thunder storm. This is reflected in the W percept in this protocol. The violence of the response is markedly reduced—"lightning" is attributed to color and "thunder" is now a less anxious concept, a favorite child's circus animal—the "elephant." This engram has form value through the protocol for the tenth year. The latter record reveals "lightning" as a comic strip and/or book symbol, thus further reducing its threatening portent. It may be inferred now that the "lightning" response is an habitual, almost automatic, association to the colored (D2) portion of Plate II. Its repetition in the future will effectively substantiate this hypothesis.

This presentation of three longitudinally persistent percepts can not answer entirely the basic question of why they have been consistently elicited in the protocols of one youngster. Despite the intimate knowledge the author has of the child, the reason (s) for the initial appearance and the repetition of the "church" engram are speculative. It does seem more plausible, however, to relate the child's learning experiences to the appearance of the percept than to ascribe a rather ingenious process of symbol-transposition—a process that is obviously too resourceful for a four year-old child.

An explanation of the other two engrams is more feasible. They originated in a dramatic anxiety-producing situation. Their continuation seemed to be assured by the author's explanation of the natural phenomenon of storms common to subtropical Florida, by the popular television programs for children, and by comic book representations. Even if the "lightning" percept does reflect anxiety in the tenth year protocol, its symbolism has been markedly mitigated. This is evidenced by the continued presence of a popular and non-threatening animal response and an adequate account of the "lightning" idea as well as its allocation to the comic book field. The latter is certainly not foreboding. Yet the question, "Why are these engrams repeated in record after record?", obstinately calls for an answer. The theoretical implications must accord with the basic rationale of the projective method—that (verbal and/or perceptual) behavior is expressive of the personality structure and personal dynamics of the respondent. The explanation for the persistence of these three Rorschach responses does not contradict the essential postulate of projective theory. It simply accounts for their origins in immediate experience and for their continuation in habituation of perception and responsivity.

The essential issue involved in this demonstration is the possibility of misinterpretation inherent in the evaluation of a single protocol. Moreover, inadequate information about a testee's background may result in inappropriate inferences, especially in regard to the deviant engrams. From a realistic and practical point of view, the dilemma does not yield to easy solution. The tester should attempt to obtain data regarding pathological percepts from sources other than the inkblot itself. This does impose upon the test user the responsibility of avoiding blind interpretations (1).

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The Association of Animals with Familial Figures

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In recent years there has been increasing interest in the contentual aspects of fantasy material. The earlier writings of Lindner (4) and Zoltan (7) emphasized interpretation of Rorschach content, and more recently there have been several attempts to score both Rorschach and TAT content in a systematic fashion (1, 2, 6).

In investigating the interpretive significance of animal contents there are two possible approaches. The first is to select criterion groups on the basis of, say roles or attitudes toward familial figures. For example one group of subjects would have loving mothers, and the other group would have punishing, rejecting mothers. Test responses would then be examined to determine whether certain animal content was produced with significantly greater frequency by one group of subjects. The problems of selecting such criterion groups would be considerable, and to the writers' knowledge, no studies of this kind have been reported.

The second approach is to investigate animal symbolism via the association method, wherein ostensible relationships between animals and familial figures are tested empirically. This method does not test directly the clinical significance of animal contents (as does the first approach). Rather it determines whether there is empirical support for the assumptions underlying interpretation of animal contents.

The purpose of the present investigation was to check out some of the symbolic interpretations that are used in connection with the Rorschach. The second approach was used, and the study is patterned after that of Goldfarb (3). He had children associate to the animal contents they had produced on the Rorschach, limiting the associations to familial or other figures (kind father, cruel woman). The subjects were also presented with a familial figure and asked to give an animal association. Goldfarb used a small number of subjects, and he presented no quantitative data. The present study is an attempt to obtain data on the animal associations of adult neuropsychiatric patients and normals. There were two experiments, each utilizing a different approach to animal associations.

EXPERIMENT I

Procedure

A list of 18 familial figures was read to the subject, one figure at a time. Each subject was instructed as follows: "I am going to read you a series of phrases, and after each one I want you to tell me the name of the first animal that comes to mind. Don't think about it. Try to answer with an animal name as quickly as possible."

The list consisted of seven mother figures, six father figures, and five child figures. The order of presentation was constant for all subjects, as follows: loving father, helpless child, timid mother, unhappy child, punishing father, dominating mother, loving mother, rejecting father, happy child, cruel mother, timid father, kind mother, wicked child, rejecting mother, cruel father, timid child, punishing mother, kind father.

Subjects

The subjects were 100 psychiatric inpatients of both sexes. The purpose of the study was not diagnostic, and therefore none of the usual descriptive statistics (age, education, diagnosis) will be presented. The subjects

TABLE I—Animal Associations (Grouped) To Various Figures ($N = 100$)
Kinds of Animals

Figures	Large Aggressive	Large Neutral	Small Aggressive	Small Neutral	Diminutives	Rejections
Father						
Loving	23	22	3.5	43.5	6	2
Kind	10	28	1	50	2	9
Timid	4	24	8.5	53.5	7	3
Rejecting	26	17	17.5	31.5	0	8
Cruel	43	14.5	16	20.5	0	6
Punishing	43.5	9.5	20.5	17.5	3	6
Mother						
Loving	6	28.5	1	54.5	9	1
Kind	3	28	2	54	12	1
Timid	3	25	2	52	16	2
Dominating	28	24	5	38	0	5
Rejecting	12	18	11	38	2	19
Cruel	31	13	11	31	2	12
Punishing	30	12.5	8	38.5	0	11
Child						
Helpless	1	2.5	3	42.5	51	0
Happy	1	5.5	2	40.5	51	0
Timid	1	7	4	44	37	7
Unhappy	10	9.5	4	43.5	27	6
Wicked	23	5.5	16	32.5	16	7

were 100 successive admissions, most of whom were acute, rather than chronic cases.

Results

Differences between the sexes were small and random, and therefore the men's and women's data were combined. The animal associations to each familial figure were so diverse that tabulating them would be meaningless. For example the associations of the first 10 subjects to timid father were: llama, bird, monkey, fox, lamb, ram, bird, horse, and rabbit. This variability is typical of the associations to all of the familial figures. It is clear that no animal is associated with any particular familial figure.

Goldfarb (3) found that adults were associated with large animals and that cruel adults tended to be associated with aggressive animals. Thus it is possible that familial figures are related not to particular animals but to classes or types of animals. In order to test this possibility animals were classified into five types: large aggressive, large neutral, small aggressive, small neutral, and diminutives (calf, puppy, etc.). Seven judges were used, and the animals were categorized on the basis of a decision by five of the seven judges. Animals that were not classified as aggressive or neutral by a clear majority were skunk, mink, porcupine, and badger. Also the following animals were not judged clearly to be large or small: hog, sow, pig, and seal. In these instances an animal association was scored half in one category and half in the other.

If a subject did not respond to the verbal stimulus in 30 seconds, it was scored as a rejection. The associations were grouped into five kinds of animals and rejections. These data are presented in Table 1.
Each row of the table adds up to 100, the number of subjects.

Because of the difficulties involved in testing the significance of the obtained frequencies no tests of significance were computed.[1] Inspection of

[1] The same 100 subjects gave their associations to all the familial figures. Since the frequencies of animal associations to the various figures were not independent, chi-square could not be used. However, the trends in the data seem clear enough for interpretation, despite the absence of statistical tests.

the columns in Table 1 reveals that a consistently high number of small neutral animals were given as associations to the various parental figures. Since the small neutral category includes most domesticated pets (cat, dog, rabbit, etc.), this response set is not surprising.

The large aggressive column reveals a clear-cut trend for both paternal and maternal figures. The frequency of large aggressive animals in response to positive figures (loving, kind, and timid) is somewhat less than the frequency in response to negative figures (rejecting, cruel, punishing). The same trend may be seen in the small aggressive column. The large neutral and small neutral columns reveal an opposite trend, although the differences are not as marked. There are fewer aggressive animals given to positive parental figures than to negative figures. The diminutives column reveals a similar pattern, positive parental figures eliciting more diminutives than negative parental figures. No other trends emerge from inspection of the animal associations to paternal and maternal figures.

The data for child figures reveal one major trend. In addition to the response set for small neutral animals, there were a large number of diminutives. In fact small neutral and diminutive animals accounted for virtually all associations to child figures.

The data in Table 1 can be summarized briefly. Aggressive animals are more closely associated with negative parental figures than with positive parental figures. Neutral animals are more closely associated with positive parental figures than with negative parental figures. Small neutrals and diminutives are associated with children. There is nothing new nor startling in these results, but it is reassuring to have empirical verification of cultural stereotypes that are sometimes used in interpreting animal content.

It would be of greater potential use to discover relationships between specific parental figures and specific animals and perhaps to verify some hypothesized relationships. For example Phillips & Smith state that the response *bear* on the Rorschach " . . . tends to be associated with a benign and sympathetic father figure . . . " (5, p. 120). In Experiment I it was found that kind father and loving father both elicited a wide variety of animal associations. There were so many different animals given in response to each of these stimuli that it was not possible to test the hypothesis that *bear* was associated with them. The sheer variety of responses is presumptive evidence against the hypothesis, but a better test would be to use *bear* as the stimulus and determine if any parental figure is associated with it. This procedure was used in Experiment II.

Experiment II

Procedure

On the basis of the results of Experiment I and clinical hypotheses, 24 animals were chosen as stimuli. Since the focus of the study was to be on parental figures, six parental figures were chosen as responses. Instead of naming a parental figure in response to each animal, the subject chose one of the six figures as being most closely associated with the animal. This method guaranteed large enough frequencies among the parental figures for hypotheses to be tested.

Selection of the six figures was based on the results of Experiment I. Loving mother, loving father, cruel mother, and cruel father yielded different kinds of animal associations, and they appeared to represent extremes of parental figures. Therefore they were selected as responses. Examination of Table 1 revealed that the adjective *dominating* represents a midpoint between loving and cruel, and therefore dominating mother and dominating father were selected as the fifth and sixth figures.

TABLE II—Frequency of Normals' Animal Associations ($N = 100$) *

	Loving Mother	Dominating Mother	Cruel Mother	Loving Father	Dominating Father	Cruel Father
Bear	37	10	1	6	21	25
Lion	21	17	7	3	32	19
Eagle	17	16	11	10	28	17
Ape	24	9	2	8	30	27
Wolf	17	8	6	6	17	45
Alligator	13	6	12	4	30	33
Octopus	11	23	15	5	21	23
Bull	4	3	1	7	50	35
Cow	80	8	1	3	3	2
Horse	40	7	0	29	16	8
Elephant	31	7	1	14	43	3
Deer	76	5	1	12	5	1
Snake	15	8	18	4	8	46
Spider	17	21	18	3	6	34
Crab	10	21	9	7	22	31
Frog	25	14	12	17	21	9
Chicken	61	18	5	1	9	4
Fish	43	15	8	17	8	8
Bug	24	18	17	8	11	20
Bird	66	17	4	8	3	0
Dog	29	9	0	24	16	19
Cat	53	27	11	0	2	6
Moth	47	10	7	13	8	10
Rabbit	80	5	3	8	1	2

*The rows in Tables II and III do not all add up to 100 because some subjects did not answer every item.

TABLE III—Per Cent of Patients' Animal Associations ($N = 50$)

	Loving Mother	Dominating Mother	Cruel Mother	Loving Father	Dominating Father	Cruel Father
Bear	40	8	2	6	16	28
Lion	22	18	12	8	14	26
Eagle	38	12	12	8	20	10
Ape	28	16	6	6	20	24
Wolf	8	20	18	6	22	26
Alligator	28	16	12	6	18	20
Octopus	16	26	18	2	20	16
Bull	6	8	4	14	30	38
Cow	82	8	6	0	2	2
Horse	42	4	4	30	14	6
Elephant	26	16	4	18	32	4
Deer	62	8	0	16	10	4
Snake	20	16	14	0	14	36
Spider	20	16	22	4	14	24
Crab	12	14	8	12	24	34
Frog	30	14	4	12	24	14
Chicken	70	14	2	2	10	2
Fish	34	18	8	28	6	6
Bug	20	10	24	6	24	14
Bird	54	14	14	22	6	0
Dog	56	4	2	16	10	12
Cat	50	14	18	8	4	2
Moth	34	14	18	16	12	6
Rabbit	62	8	6	20	0	2

Subjects

The subjects were 100 normals and 50 psychiatric inpatients of both sexes. The patients were different from those used in Experiment I. The normals consisted of 75 college students and 25 psychiatric aides.

Results

The results are presented in Tables II and III. Examination of the results yielded no systematic differences between college and noncollege subjects, and their data were combined. The patients' data were converted into percentages so that they would be comparable to the normals' data which were based on an *N* of 100. The data of patients and normals are strikingly similar, the differences being small and random. The following account holds for both patients and normals except where noted.

It seems reasonable that an association should be made by at least half the subjects if it is to be useful. Therefore a frequency (or percentage) of 50 was adopted as criterion of meaningfulness.[2] According to this criterion loving mother is meaningfully associated with cow, deer, chicken, bird, cat, rabbit; dominating father is associated with bull by normals only. Patients associate loving mother with dog, but college students do not. No other associations met the criterion of 50%.

Tables II and III are arranged vertically in the following order: eight large aggressive, four large neutral, three small aggressive, and nine small neutral animals. Inspection of the columns (parental figure response) reveals some interesting trends. The frequency of the response loving mother tends to be higher for neutral animals than it is for aggressive animals. For loving father the same trend is present but somewhat weaker. These trends are reversed for cruel father, with frequencies tending to be higher for aggressive animals than for neutral animals. The frequencies for dominating mother and father were intermediate between loving and cruel. These trends are essentially the same as those found in Experiment I.

Discussion

The results of both experiments show that few parental figures are associated with specific animals. The only parental figure that both normals and patients associate with specific animals is loving mother. There is empirical verification for the contentions by Phillips & Smith (5) that cow and deer are associated with a kind, loving mother. Their association of bull with dominating father and of diminutive animals with children are also corroborated. However, their postulated associations of loving father with bear and horse, of cruel or dominating father with ape and eagle, and of cruel or dominating mother with spider and octopus all failed to stand up in the present study.

These results suggest that the clinician should beware of making interpretations on the basis of individual animal contents. Evidently there are too few specific cultural stereotypes to allow for such interpretation. On the other hand when animals are grouped by size and aggressiveness, cultural stereotypes do emerge. Punitive parents tend to be associated with aggressive animals and kind parents with neutral animals. Children tend to be associated with small neutral animals or the young of animals. These results suggest that groups of animal contents might be of interpretive significance where single contents would not.

The data of these two experiments corroborate Goldfarb's results (5). He found that there were diverse familial associations to any single animal but

[2] As in Experiment I the frequencies are not independent, and chi-square could not be used. In any event determining whether the frequencies deviate significantly from chance would not be a stringent enough test. Unless a majority of subjects associate a single parental figure with a given animal, the relationship would not be meaningful in clinical situations.

that aggressive animals tended to be associated with cruel parents, passive animals with kind parents, etc. Since the subjects in the present study were adult normals and psychiatric patients and Goldfarb's subjects were adolescents seen in an outpatient clinic, these results would seem to have considerable generality. Taken together, they argue against attributing "deep" symbolic meanings to animal contents. Rather animal contents should perhaps be interpreted in terms of general cultural stereotypes. Such stereotypes probably develop in two ways. First where the attributes of the animal are extreme, e.g., ferocity in the lion, gentleness in the deer, these characteristics may be associated with types of familial figures. Second colloquialisms and folk-sayings foster animal associations, e.g., "snake in the grass," "stubborn as a mule," "timid as a rabbit." Basic research on such cultural stereotypes might prove to be of value in the clinical interpretation of content on projective techniques.

SUMMARY

Two experiments were performed to determine animal associations to familial figures. In the first experiment psychiatric inpatients gave their first animal association to each of 18 figures (maternal, paternal, and child). In the second experiment psychiatric inpatients and normals gave their parental associations (via a forced choice technique) to 24 animals. Both experiments revealed that there were few familial figures associated with specific animals. Rather, it was found that animals grouped by size and aggressiveness were related to familial figures. These results suggest caution in interpreting animal contents in clinical situations.

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Characteristics of the Thematic Apperception Test Heroes of Normal, Psychoneurotic, and Paranoid Schizophrenic Subjects

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The present study is an outgrowth of a larger project (5) in which the characteristics of Thematic Apperception Test (TAT) heroes were compared with phenomenological self and ideal self pictures. It is the purpose of this paper to report the manner in which the different groups tended to characterize their story heroes with particular focus upon the positive or negative qualities attributed to such figures.

METHOD

Eighty statements were prepared from the TAT hero characteristics that appeared in a random sample of protocols from people similar to the experimental population and from the common themes to TAT cards as reported by Stein (7). The statements were derived from stories to cards 1, 3BM, 6BM, 7BM, and 14, and reflect the traits and attributes, feelings and emotions, experiences, tendencies toward behavior, goals and orientations that people generally attribute to their TAT heroes in varying degree.

In the present study, the Q-sort rating technique (8) was adopted to give a single composite description of each individual's TAT hero. It was the task of E. to sort these 80 statements for each protocol into 9 piles approximating a normal distribution and ranging from those statements which were most characteristic of the TAT hero to those which were least characteristic. Since E. was the only rater in the research proper, a reliability study was necessary to show that other judges could sort the statements, from reading the TAT protocols, in a similar manner. This study, reported in more detail elsewhere (6),[1] yielded an inter-rater reliability coefficient of .75. In addition, each S sorted these same statements for an ideal self.

The sample consisted of 48 white, male Ss—16 normals, 16 psychoneurotics, and 16 paranoid schizophrenics. The three groups were roughly equated as to age and education. The paranoid schizophrenic group consisted exclusively of hospitalized patients with a psychiatric diagnosis of paranoid schizophrenia at the time of testing. The neurotic group was composed of non-hospitalized Ss who were undergoing some form of psychotherapy. All but one member of this group were veterans of World War II who had received a "service-connected" diagnosis of psychoneurosis and all had an existing neurotic diagnosis at the time of testing. No one was included in this group if a previous diagnosis of psychosis had been made. The utilization of a group undergoing psychotherapy was considered feasible since, in all cases, the number and type of contacts were not sufficient to produce any marked change. At the time of examination, the Ss were deemed representative of a neurotic population. The normals were people who seemed to be functioning effectively socially and occupationally, who felt no serious maladjustment within themselves, and who had no history of psychiatric treatment.

RESULTS

Since the ideal self sort represents a

[1] This article also contains a complete listing of all the statements employed in the present study as well as a detailed account of the method used for rating each TAT protocol.

picture of qualities which an individual perceives as desirable, the relationship between TAT hero characteristics and the ideal self may be viewed as an indication of the positive or negative quality of the TAT hero. The correlations between TAT hero and ideal self were computed by means of the product-moment correlation formula suggested by Cronbach (1) for use when the scores of all persons have the same mean and variance as in the case of Q-sort data. The individual intercorrelations of each person in each of the three groups are presented in Table I along with the median intercorrelations of the three groups.

TABLE I—Correlations Between the Ideal Self Sort and the T.A.T. Sort for Each Subject in the Three Groups

S	N*	PN*	PS*
1	.69	.17	—.38
2	.47	.36	—.36
3	.15	—.32	.29
4	.36	.05	.04
5	.50	.12	—.02
6	.33	—.19	.02
7	.19	—.31	—.02
8	.39	—.06	.01
9	.33	—.03	.07
10	.48	.08	.06
11	—.05	—.12	.43
12	.27	—.19	.02
13	—.09	—.21	—.01
14	.43	.19	.24
15	—.12	—.03	—.20
16	.62	.06	—.11
Median	.375	—.03	.015

* N = Normal, PN = Psychoneurotic, PS = Paranoid Schizophrenic.

The significance values computed by means of the Wilcoxon "T" test (10) indicate that the ideal TAT correlations of the normal are significantly different (and higher) than those of the neurotics and paranoid schizophrenics at the .01 level of confidence, whereas no significant differences are in evidence between the two pathological groups. These results point to the fact that the characteristics of the TAT hero of the normals more closely resemble the ideal self concept than in the other groups.

These findings fail to indicate whether the observed differences are related to systematic differences between the groups in the ideal self concept or in the TAT.[2] An attempt was made, therefore, to examine TAT differences more closely. The significance of the differences between the means for each of the 80 items of the TAT sort was computed for each group compared with every other group. The observed number of significant items were compared with the number expected by chance using chi-square methodology and the results are summarized in Table II. These results indicate systematic differences, greater than chance expectancy, between the TAT hero characteristics of normals as compared to each of the pathological groups but no systematic differences between the TAT hero characteristics of the two pathological groups.[3]

TABLE II—Observed Number of Significant Items, Chi-Square Values, and P-Values for the T.A.T. Sort of Each Group Compared with Every Other Group at the .05 Level of Confidence

Groups	No. Significant Items	Chi-Square Values	P
N-PN	25	110.59	.01
N-PS	16	34.80	.01
PN-PS	5	.065	.80

A clearer picture of the nature of the differences between the groups

[2] Data presented elsewhere (4) indicates that no substantial differences exist in ideal self concept in the different groups. This finding is in essential agreement with other investigators (3, 9) utilizing different statements and populations and seems to suggest that the ideal self concept represents, in large measure, a cultural stereotype.

[3] Since the comparison between the psychoneurotic and paranoid schizophrenic groups failed to reveal systematic differences, further comparison between these groups is omitted.

may be obtained from an examination of the significant items. These items are presented in Tables III through VI. Since a statement may be significant because it is *more* characteristic of the TAT hero in one group than in another, this does not necessarily imply that it is characteristic or not characteristic of the TAT hero in absolute terms. For this reason, Tables III through VI present the means of the significant items for the readers' reference. A mean of 4.0 represents "neutrality" whereas a higher mean tends to be more characteristic of the TAT hero and a lower mean less characteristic.

TABLE III—T.A.T. Sort Items Significantly More Characteristic of the Normals Than the Neurotics at the .05 Level of Confidence

Item No.	Mean N	Mean PN	Item
4.	5.7	4.4	capable
7.	6.5	4.0	gets very interested in things
10.	5.0	3.5	has a good imagination
14.	2.5	1.4	gambles
47.	5.5	3.8	thinks about enjoyable things
51.	5.0	2.4	looks at the bright side of things
54.	5.5	4.3	daydreams a good deal
59.	5.1	3.0	obeys parents
75.	4.3	2.0	follows father's advice
76.	4.5	2.8	feels close to father
78.	5.5	3.9	respects father
79.	4.1	1.9	helped by father's guidance

Scanning Tables III through VI, it appears that the statements characteristic of the TAT hero in the normal group as compared to the two maladjusted groups tend to reflect positive qualities and attributes. To evaluate this quantitatively, we selected 23 "desirable" and 23 "undesirable" statements. "Desirable" statements were considered to be those in which the mean for this sorting in all three groups was 5.0 or greater. "Undesirable" statements were those with a mean of 3.0 or less for all three groups. An examination of the significant statements in this light revealed that 10 desirable statements were more characteristic of normal TAT heroes than neurotic heroes; 11 desirable statements were more characteristic of normal TAT heroes than paranoid schizophrenic heroes; 10 undesirable

TABLE IV—T.A.T. Sort Items Significantly More Characteristic of the Neurotics Than the Normals at the .05 Level of Confidence

Item No.	Mean PN	Mean N	Item
3.	5.0	2.9	discouraged over lack of success
23.	4.1	2.8	has been a disappointment to father in some respects
29.	5.1	2.3	becomes discouraged easily
31.	6.6	4.4	feels blue and downhearted
34.	4.9	3.1	displeased with home life in some respects
38.	5.9	4.1	upset by things which later seem unimportant
43.	4.3	2.2	feels bitter toward people
44.	3.3	1.6	tries to get even with people
57.	5.9	4.0	forced to do unenjoyable things
61.	4.6	2.1	often wronged by other people
62.	3.7	2.4	was punished by father
63.	3.9	2.8	was punished by mother
64.	3.9	2.5	has felt angry at father

TABLE V—T.A.T. Sort Items Significantly More Characteristic of the Normals Than the Paranoid Schizophrenics at the .05 Level of Confidence

Item No.	Mean N	Mean PS	Item
1.	5.9	3.9	ambitious
2.	6.1	3.8	works hard to achieve a desired goal
4.	5.7	3.9	capable
7.	6.5	4.9	gets very interested in things
36.	4.8	2.7	gets over disappointment quickly
47.	5.5	4.2	thinks about enjoyable things
48.	6.3	5.0	thinks how nice it would be to be a success
51.	5.0	2.9	looks at the bright side of things

TABLE VI—T.A.T. Sort Items Significantly More Characteristic of the Paranoid Schizophrenics Than the Normals at the .05 Level of Confidence

Item No.	Mean PS	Mean N	Item
3.	5.2	2.9	discouraged over lack of success
9.	4.3	3.1	envies others
15.	4.9	3.9	stubborn
29.	5.4	2.3	becomes discouraged easily
37.	5.8	4.5	sometimes feels like crying
43.	4.2	2.2	feels bitter toward people
62.	3.4	2.4	was punished by father
63.	3.6	2.8	was punished by mother

items characterized neurotic heroes as compared to the normal; and 8 undesirable items characterized the paranoid schizophrenic heroes as compared to the normal. No desirable statements were significantly more characteristic of the TAT heroes of the two maladjusted groups, when compared with the normal group, and no undesirable statements were significantly more characteristic of normal TAT heroes than of neurotic and paranoid schizophrenic heroes. These results clearly reveal the relatively more positive nature of the TAT hero in a normal group as compared to neurotic and paranoid schizophrenic groups. In particular, it appears that the TAT hero characteristics of the normals reflect greater feelings of adequacy, satisfaction, and optimism about life, as well as a less stressful relationship with people and parents. On the other hand, the TAT hero characteristics of the psychoneurotics and, somewhat less strikingly, of the paranoid schizophrenics, are marked by stronger depressive qualities, less satisfaction with life, and more strained relationships with people and with parents.

A final issue to be considered is the question of homogeneity. In any group comparison research it is helpful to know how much alike are the members of one group and how much different they are from members of another group. Toward this end, within and between groups correlations were obtained by the method of group correlation suggested by Cronbach (1). The results are summarized in Table VII.

TABLE VII—Within and Between Groups Correlation for the T.A.T. Sort

Within		
N	PN	PS
.261	.245	.119
Between		
N-PN	N-PS	PN-PS
.114	.110	.154

While none of the correlations in Table VII is exceptionally high, it should be pointed out that the absolute size of correlations based upon a Q-technique study is not interpretable in the same way as any of the traditional correlation coefficients (2). The important aspect is the relative magnitude of these correlations. From Table VII it may be observed that the mean within groups correlations of both the normal and neurotic groups are appreciably higher than that of the paranoid schizophrenic group, revealing the greater variability of the paranoid schizophrenic group in their characterization of the TAT hero. The higher correlation between the neurotic and paranoid schizophrenic groups suggest a greater similarity between these two groups and a greater relative dissimilarity to the normal on the TAT.

To verify these observed trends, within and between groups correlations were compared for statistical significance by the Wilcoxon Matched Pairs Signed Ranks Test (10). The results are summarized in Table VIII and indicate the relative heterogeneity of the paranoid schizophrenic TAT.

SUMMARY

An 80 statement Q-sort was used to

characterize the TAT hero of each subject who, in addition, sorted these same 80 statements for his ideal self-concept. The sample consisted of white, male Ss—16 normals, 16 non-hospitalized psychoneurotics, and 16 hospitalized paranoid schizophrenics.

The findings of the present study may be summarized as follows:

1) The TAT hero of the normal correlates with the ideal self concept to a significantly greater degree than the TAT hero of a psychoneurotic and paranoid schizophrenic population.

2) The normal TAT hero is significantly different from the TAT hero of the psychoneurotic and paranoid schizophrenic groups whereas the two maladjusted groups do not differ significantly from one another.

3) The TAT hero of the normal reflects greater feelings of adequacy, satisfaction, and optimism about life, as well as a less stressful relationship with people and parents, and less depressive qualities when compared with the TAT hero of neurotic and paranoid schizophrenic Ss.

4) While the TAT hero in all groups revealed variability, both the normal and psychoneurotic TAT hero were sufficiently homogeneous to achieve statistical significance whereas the paranoid schizophrenic TAT hero proved most variable and relatively heterogeneous.

TABLE VIII—Significance Values for the Within and Between Groups Comparisons for the T.A.T. Sort

Within Group	Between Groups	P	P/2
N	N-PN	.05	.025
N	N-PS	.10	.05
PN	PN-N	.10	.05
PN	PN-PS	.07	.035
PS	PS-N	.33	.165
PS	PS-PN	.21	.105

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The Effects of Warm and Cold Interaction on the Interpretation of a Projective Protocol[1]

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An increasingly important area of research in the use of projective techniques concerns the influence of the examiner-subject interaction on the results of the diagnostic interview. Clinicians are now aware that testing involves a relationship between two people, neither of whom are standardized or constant. The awareness of the importance of the interaction, however, has not as yet led to any appreciable body of unequivocal research results. Klopfer et al, for example, state that "any holistic interpretative procedure should take account of the subject-examiner relationship as just as much part of the total context as the stimulus material itself. However, the influence of this aspect of the test situation has been inadequately explored" (8, p. 349).

What experimental evidence there is relating to this problem is contradictory. Various investigators (1, 3, 4, 5, 10, 13) have found that Rorschach examiners differ from each other in the kinds of responses given by their subjects. Other findings, however, (2, 7) indicate that differences in examiners do not produce significantly different responses in subjects. The research is not only contradictory, but much of it is subject to serious methodological criticism, as Levy (9) and Hammond (6) have indicated.

The research in this area has focused on the influence of the examiner on the behavior of the subject. There has been no experimental work on the influence of the subject on the examiner's behavior. Yet as Schafer (14) has suggested the testing situation is characterized by the mutual interplay of examiner and subject expectations, wishes and needs. The present study was designed in an attempt to investigate the examiner's behavior as it was influenced by the subject. The method that was employed was the use of accomplices, posing as test subjects, who assumed warm and cold roles toward the examiner.

The specific hypotheses which were investigated were as follows:

I. When an examiner tests several subjects he will make more positive statements about the subjects who like him than those who do not.

II. When a subject acts interested in an examiner more positive statements will be made about him than when he acts disinterested.

METHOD

The examiners in this study were the eight graduate students in the author's seminar in projective techniques. Three of the students had used projective tests previously, but this had been the first formal course in the area for all of them. The experiment was introduced during the last month of the semester, by which time each student had received supervision in administering and interpreting seven TAT's and several Incomplete Sentence protocols.

They were told that Dr. Rotter, the author of the Rotter Incomplete Sen-

[1] The author is greatly indebted to the members of his class who served as examiners: Herbert Dandes, Everett Gertner, Marie Gill, John Hertzberg, Calvin Mosher, Arthur Schatz, David Sine and June Tapp. The success of the study was due to the ability of Carol Baker and Jane Nolan to act so convincingly as the accomplices. The statistical procedures used were discussed with Dr. Jerome Schiller and Dr. Norman Wallen. The judges were Dr. Bernard Braen, Dr. Jerome Schiller, Mr. Morton Silverman and Dr. Matthew Trippe.

tence Test, was engaged in a nationwide effort to gather information ragarding the manner in which his test was used in various installations and clinics. They were also told that Rotter wished to develop better norms for his test, using all varieties of subjects. This being the case, they were asked not to communicate with each other regarding the subjects they saw or the manner in which they interpreted the case. A copy of their reports would be sent to Dr. Rotter, they were told. The class accepted this explanation without question, all eight students agreeing to participate in the study.

The test subjects were two "accomplices", both attractive undergraduate girls who volunteered to take part in the experiment as part of the requirement of their introductory psychology course. Both accomplices were given practice in assuming warm and cold roles toward the examiner. In the warm role the accomplice was told to act friendly and interested in both the testing situation and the examiner, the goal being to make him feel comfortable and accepted. In the cold role she was to act in a formal, disinterested way and her object was to make him feel awkward and incompetent. The accomplices were warned that the examiners were relatively sophisticated in assessing other people, and they were urged not to "ham it up."

Two Incomplete Sentence protocols were prepared by using the examples given in the Rotter-Rafferty Manual (12). Each accomplice was given one protocol to memorize and they were both warned of the necessity of presenting the same responses to each examiner. An analysis of the protocols showed that each accomplice did, in fact, respond identically to the eight examiners. In writing the two fictitious protocols, the aim was to give this material an over-all neutral quality, so that it would not appear to the examiner to be either an obviously healthy or sick record. To achieve this goal, there were equal numbers of very sick and very healthy responses selected, mildly sick and mildly healthy responses, etc. Each protocol contained two sentence endings that would be given a score of six in Rotter's system ("I feel . . . depressed at times") and two endings that would be scored zero ("The best . . . time is right now"); four responses that would be scored five ("I am best when . . . I know that people approve of me") and an equal number that would be scored one ("At bedtime . . . I fall right to sleep"); eight responses that would be scored four "My greatest fear . . . is of cancer") and eight that would be scored two ("The happiest time . . . will be when I get married"); and twelve neutral endings that would be given a score of three ("Back home . . . In Indiana"). Each protocol, therefore, while containing different stem endings, had a mean conflict score of three, the neutral category. For the purpose of this study, the protocols were considered alternate forms.

Each examiner saw each accomplice in an individual session. By a prearranged schedule, one accomplice would be warm to a particular examiner, while the other would be cold. Each examiner, therefore, interacted with a cold subject and a warm subject. To counter balance any possible effects of the protocols used, each accomplice had an equal number of cold and warm roles. In responding to the sentence stems, the accomplices were instructed to answer in a nonrehearsed manner and to skip some of the stems the first time they were encountered but to answer them later.

For the most part the accomplices were able to convey the impression that their responses were genuine and not faked. Unfortunately, part way through the study one of the examiners saw an accomplice whom he had already tested go into the testing room with another examiner. He communicated this to the second examiner and ultimately a third examiner was informed that some of them were see-

ing the same subjects. In addition, two of these examiners learned that some of the sentence endings were identical in their protocols. The author was informed that rumors were being circulated regarding the reason for the study and at the end of the experiment, but before the purpose of it was revealed to the class, he requested the examiners to enumerate the facts and theories they had regarding the experiment. One S (Examiner B) believed completely the reasons for the study that the author gave in class, another (Examiner D) became suspicious of the rapid manner in which one of the accomplices had responded to the stems but had no information or theory concerning the nature of the study, and three S's (Examiners C, E and H) were not aware that the protocols were faked but had private thoughts regarding the interest the instructor had in the examiner as a variable in projective testing. The three S's (Examiners A, F and G) who had learned something about the use of accomplices concluded that the author was interested in determining the reliability of interpretation of a projective protocol. Thus, none of the S's had anticipated the purpose of the study, even though six of them were convinced that they were the real subjects, rather than the examinees. However, since none of them knew which aspect of their behavior was of interest to the author it may be assumed that their interpretations were written independently of any knowledge of the specific hypotheses of the study.

Each of the 16 interpretations was divided into thought units by the author. Any sentence, or part of it, which expressed a new idea was considered to be a separate unit. Enumerations and synonyms ("She is insecure and anxious") were considered single units. There was a total of 711 units in the 16 protocols, with a range of 20 to 79 per protocol, a mean of 44 and a median of 39. After each interpretation had been separated into units, all references to the examiner-subject interaction were removed, the names of the examiner and subject were coded, and judges were asked to determine for each unit whether the examiner was making a "positive", "negative" or "other" statement about the subject.

Positive statements were defined as indicating one or more of the following characteristics about the subject: she is in good psychological health; she is moving in the direction of good psychological health; she has insight into her problems; she has made an investment in some activity or interest; she is motivated to relate well to others. Negative statements were defined as the obverse of the above characteristics (poor health, lack of insight, etc.). Other statements were those that could not be classified along the positive-negative dimension ("She is a fine arts major"). The judges were given practice in the scoring of practice units, after which each judge independently scored the 16 protocols. The first two judges who attempted this were not given sufficient practice and confused the phenomenological approach, which the instructions called for, with the more usual depth, or clinical, approach. As a result, two new judges were selected, the training period was lengthened and the instructions were clarified. After the independent judging of the protocols, the judges discussed the 133 units where they had differed until they could both agree on a rating. Since the author was aware of the identification of each protocol, he did none of the judging and he did not attempt to influence the judges in their ratings.

RESULTS

The two judges independently agreed on the categorization of 578 of the 711 units, an agreement of 81%. When the protocols were placed in rank order in terms of percent agreement, the range of agreement was from 59% to 96%, with a mean and median of 77%.

After both judges had agreed on a

Table I. The Number and Percentage of Positive, Negative and Other Statements by Examiner, Accomplice and Condition

Exmainer	Accomplice 1 Warm	Accomplice 1 Cold	Accomplice 2 Warm	Accomplice 2 Cold
A	Pos. 25 (36%) Neg. 38 (55%) Other 6 (9%) Pos/Neg .66			Pos. 20 (29%) Neg. 43 (61%) Other 7 (10%) Pos/Neg .48
B		Pos. 22 (28%) Neg. 51 (65%) Other 6 (7%) Pos/Neg .43	Pos. 40 (67%) Neg. 16 (27%) Other 4 (6%) Pos/Neg 2.48	
C		Pos. 18 (49%) Neg. 11 (30%) Other 8 (21%) Pos/Neg 1.63	Pos. 23 (66%) Neg. 9 (26%) Other 3 (8%) Pos/Neg 2.54	
D	Pos. 16 (35%) Neg. 21 (57%) Other 4 (8%) Pos/Neg .61			Pos. 9 (41%) Neg. 12 (55%) Other 1 (4%) Pos/Neg .75
E		Pos. 9 (28%) Neg. 21 (66%) Other 2 (6%) Pos/Neg .42	Pos. 15 (63%) Neg. 7 (29%) Other 2 (8%) Pos/Neg 2.17	
F	Pos. 5 (20%) Neg. 16 (64%) Other 4 (16%) Pos/Neg .31			Pos. 5 (17%) Neg. 14 (48%) Other 10 (35%) Pos/Neg .35
G		Pos. 5 (25%) Neg. 12 (60%) Other 3 (15%) Pos/Neg .42	Pos. 17 (41%) Neg. 17 (41%) Other 7 (18%) Pos/Neg 1.00	
H	Pos. 15 (25%) Neg. 36 (59%) Other 10 (16%) Pos/Neg .42			Pos. 8 (13%) Neg. 47 (77%) Other 6 (10%) Pos/Neg .17

single rating for each unit, the number of positive, negative and other ratings for each protocol was determined. In addition, the ratio of the number of positive ratings to the number of negative ratings for each protocol was computed. A ratio greater than 1.00 indicates more positive units than negative, while a ratio less than 1.00 indicates more negative units than positive. Table I presents the number of positive, negative and other units and the ratio of positive to negative units by examiner, condition and accomplice.

Following the suggestion of Siegel (15), Hypothesis I was tested by the use of the Wilcoxon matched-pairs signed-ranks test, a non-parametric statistic taking into account both the direction and magnitude of differences. The data used to test this hypothesis were the comparisons of the positive/negative ratios between the warm and cold conditions. An inspection of Table I revealed that only in the protocols of Examiners D and F were the ratios higher for the cold condition than the warm, the magnitude of these differences being the lowest and second lowest among the eight pairs of comparisons. This result would have occurred by chance alone between one and two and a half times in 100, using one-tailed probability values. It is evident, then, that the present examiners made more positive statements about a warm subject than a cold one.

Hypothesis II was tested by assum-

ing that the test behavior and protocol of Accomplice 2 could be considered an alternate form of Accomplice 1. The 16 ratios of positive to negative statements were ranked and divided at the median (.485) and an analysis made to determine how frequently the warm condition produced a ratio larger than the median as contrasted with the frequency with which the cold condition produced a ratio larger than the median. It was found that in the eight warm conditions, six ratios were higher, and two lower, than the median, while in the eight cold conditions, two ratios were higher, and six lower, than the median. A standard error of the difference between proportions yielded a critical ratio of 2.00, significant at the .025 level, using one-tailed probability values. It may be concluded, therefore, that when the present subjects acted warm to an examiner, more positive statements were made about them than when they acted cold.

DISCUSSION

The results of this study indicate that the interpretation of a projective protocol is in part a function of the subject-examiner relationship. The examiners used in this experiment could not separate the meaning of their interactions with the subjects from the interpretation of the protocol. Even more impressive than the quantitative analysis of the interpretations were the qualitative differences in the descriptions of the subjects. For example Examiner C had this to say about Accomplice 2, who interacted with him in a warm manner: "She feels part of her family group . . . enjoys school very much . . . seems to be very conscientious and recognizes deficiencies within herself which she is trying to remedy . . . positively oriented toward the future . . . sensitive, introspective . . . likes people". Describing the *same* accomplice and the *same* protocol, but having experienced a cold interaction, Examiner A wrote: "She has little insight or definition of her problems . . . frequently expresses considerable tension . . . goes on crying jags and feels sorry for herself . . . feels depressed . . . compulsively sets exacting standards for herself . . . lack of sympathy for others . . . uncomfortable in strongly affective situations and solves her problems by ignoring them and denying their existence."

It is apparent that these two examiners were responding to different stimuli. The protocol itself is only part of the material which the psychologist has to assemble and give meaning to; the manner in which the responses are given, and more importantly, the feelings which the interaction arouses in the examiner, obviously help determine interpretive behavior as well.

Because the protocol is such an ambiguous and unstructured stimulus, it may serve as a projective device for the examiner, as Hammer and Piotrowski (5) have indicated. In a setting as ambiguous and unstructured as an interview, a psychotherapeutic session or a testing situation, the psychologist must often rely heavily on cues which have no easily located referrent. Previous research has shown that both the interviewer (11) and the subject (17) respond to interpersonal cues of which they are unaware. In the present experiment the examiner's judgment was influenced without his awareness or control. The author did notice, however, that the students who had done the best work in his class were very sensitive to the attitudes of the subjects while the two examiners whose reports were biased in favor of the cold accomplice were the least skilled. (In view of the results of this study, it is entirely possible that the author's evaluation of his students' performance is in some measure a function of the warmth of their interaction with him, their appreciation of his small jokes, or the extent to which they provided interpretations supporting the hypotheses of this experiment.)

The present study is characterized by several weaknesses. The small number of examiners and the use of only two accomplices makes it unwise to generalize the findings to larger populations of psychologists and subjects. All that can be concluded is that using these particular subjects the hypotheses were substantiated quantitatively and qualitatively. A second weakness concerns the use of relatively inexperienced graduate students as examiners. It is interesting to speculate on the results that would have been obtained had the examiners been thoroughly experienced and sophisticated in diagnostic procedures. Pertinent to this question is the conclusion of Stanton and Baker (16) that the bias of the interviewer affected interviewer responses even with experienced interviewers who were aware of their bias and who were dealing with objective material. The third limitation in this study concerns the fact that three of the examiners knew they were testing the same subjects. It is interesting to note that of the two examiners whose interpretations ran counter to the hypotheses, one came from this group. Evidently, knowledge of the use of accomplices did not produce in these three examiners any greater tendency to write interpretations supporting the hypotheses than in the other five examiners. While the effect of this leak in information cannot be determined with any precision, it certainly weakens the confidence one can place in the findings.

Summary

1. The author arranged appointments for the eight members of his class in projective techniques to administer and interpret two Rotter Incomplete Sentence Tests. The examiners were not aware that the test subjects were accomplices, each of whom responded with faked, memorized stem endings. These protocols were constructed so that they could be considered alternate forms. Every examiner saw one accomplice who acted warm and another who acted cold and each accomplice had an equal number of warm and cold roles. The interpretations which the examiners made of the protocols were analyzed for the number of positive, negative and other statements.

2. Two judges agreed 81% in classifying responses as positive, negative or other, while the mean and median agreement per protocol was 77%.

3. The data indicated: (a) when an examiner tested two subjects he made more positive statements about the one who liked him than about the one who did not; and (b) when subjects were warm to the examiner they were perceived more positively than when they were cold.

4. Several weaknesses of this study were discussed.

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A Note on the Use of the Schizophrenic in Rorschach Content Analysis

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Introduction

Rorschach content interpretation is freely practiced in clinical work today; however, Rorschach's original disparagement has retarded development of a dynamic theory of content analysis. The usual approach employed by psychologists is the highly subjective procedure of free associating to the patient's responses to arrive at their meaning. Some clinicians compensate for their felt inadequacies through the assignment of invariant meanings to specific images without reference to individual differences, much as they might rely upon universal symbolism in dream interpretation. Lindner (4), a leading exponent of this extreme view, correlated single, specific responses with major pathological trends and diagnostic categories. Other authorities, such as Shafer (9), have reacted strongly against this inflexibility of interpretation, advocating a thematic analysis of content, in much the same sense as applied to TAT stories, but cautioning that interpretations are only hypotheses and remain in need of careful internal and external validation.

The difficulty encountered in the scientific investigation of the unconscious and symbolic aspects of human behavior has further limited research in this area. Psychoanalytic theory suggests a novel avenue of approach through the occasional, fascinating and informative schizophrenic who retains the ability and willingness to introspect for the interested clinician. Fenichel (2,p.422) aptly represents the psychoanalytic viewpoint in his statement that because the "unconscious has become conscious", the psychotic is dominated by archaic modes of thinking. "The schizophrenic shows an intuitive understanding of symbolism. Interpretation of symbols, for instance, which neurotics find so difficult to accept in analysis, are made spontaneously and as a matter of course by the schizophrenic." He often expresses ideas which are deeply repressed in others; that is, the expressed thoughts of the schizophrenic may be similar to the unconscious thought of the normal or neurotic. Symbolic thinking for them is not merely a method of defensive distortion; it is an archaic pictorial type of thinking that occurs in all regressive states, such as fatigue, illness, sleep and psychosis. Many psychoanalysts are known to conclude that any patient who could correctly interpret his own dreams was schizophrenic. Brill (1) states, for example, that he never found a patient not schizophrenic who could interpret his own symbols correctly. Rosen (8) thinks enough of the schizophrenics' facility in understanding psychotic behavior and verbalizations that he has employed patients-in-remission to advise him in treating severely disturbed psychotics. He literally equates schizophrenia with dreaming and sees the problem of treatment as that of waking the patient from his nightmare.

Procedure

Schizophrenic patients were employed to interpret the very private, individualistic, fleeting, mutable symbols frequently represented in Rorschach responses. Over a period of two years, approximately 50 schizophrenic patients were informally screened for participation in this study, and three were selected on the basis of their willingness and demonstrated ability to cooperate in the interpretation of a

variety of symbolic content. Each manifested behavior suggestive of an intimate acquaintance with symbolic modes of thinking, though care was exercised that they had received no formal training, including psychotherapy, in the interpretation of symbolic phenomena. Their prior familiarity with the Rorschach method of personality appraisal was limited to having taken this test upon admission to the hospital. Patient A was brought to the attention of the author by his elaborate, detailed religious-symbolic paintings in occupational therapy. Patient B suggested himself as a subject when he volunteered interpretation of another schizophrenic's autistic ward behavior. The third patient, Subject C, was included when he spontaneously interpreted the symbolic meaning of his own responses during the administration of a Rorschach.

Each subject was given the same five Rorschachs for interpretation; all protocols were obtained from young male subjects, three from schizophrenics, one from a hospitalized neurotic, and one from a "normal" college student. The validating criteria consisted of a case summary consolidated from an intensive psychiatric, psychologic, and social service evaluation, and the cross validation provided by the three interpretations of each Rorschach. The study concerned only the interpretation of content, divorced from other aspects of the response, such as location or determinants. The method of "blind analysis" was partially adhered to, in that only the sex and age of the Rorschach respondents were provided.

Results

Results obtained from the three patients were highly encouraging; the patients associated freely and made interpretations of symbolic content with an attitude of considerable self-assurance. The apparent ability of these untrained subjects to arrive independently at similar interpretations derived from Rorschach content analysis is illustrated by the following example:

Rorschach protocol (IV) presented to the patient-subjects:

Card:

I
1. A bat.
2. A dried leaf, disintegrating or rotting.

II I never saw or thought anything like this.
1. Blood, perhaps. I certainly don't see anything there. Could be a picture of a pelvis bone with blood on it. But blood is in the wrong spot; should be in the middle or no blood at all. I was a pharmacist's mate in the Navy.

III
1. Looks like autumn to me, dried leaves again. Two colors, brown and red. It really doesn't look like anything.

IV I don't get anything out of that one.
1. Could be a turtle under water. Looks encrusted.

V
1. Could be a bat.

VI
1. Looks like a bone, a hip bone, but it's way out of shape.

VII It's like nothing I ever saw.

VIII
1. Looks like two moles, the two pink items, crawling over some rocks. Like at the zoo. Could I read the back, doctor?

IX
1. (Slants card). Like coal put in water in the wintertime. But doesn't really look like it.

X
1. Tree stump.
2. Young tree, a sprig, ready to be stuck in the ground. Possibly a book print or magazine illustration.

Verbatim recorded interpretation of Patient B:

I
1. An attempt at flight or escape from the test.
2. The tree is a symbol of life. Every leaf gets its vitality from the tree and is dependent on it. The tree could be his family or society. This is an individual member that has fallen off into a mental institution. He feels he is rotting in the hospital, isolated from others.

II
1. He begins by denying what is to follow. He tries again at the end to deny its importance by saying he was a pharmacist's mate. It is clear that this is the pelvic bone of a

female, since he says the blood should be in the middle. He is possibly preoccupied with obscene sex thoughts.

III 1. Autumn and leaves?! Autumn is in the fall, that is, late in life. Dried leaves again indicates loss of life. He is saying, "It is late in life for this to happen to me. My life is wasting away."

IV 1. Underwater means death. Deep water means danger or trouble. He is a guarded creature, living in hidden depths. The turtle is a cautious animal who finds security in its shell and underwater, hidden away. 'Encrusted' means being weighted down with anxiety or trouble. He has been like this for a very long time.

V 1. Another guarded response.

VI 1. Repetitious. His concern with hips shows an interest in sex. As on the second card, there is something wrong, that is, the blood in the wrong spot or the hip bone is misshapen. He may have a problem about homosexuality.

VII The card represents some great danger —it is too close to his 'trouble area.' Maybe this is a carryover from the last card. Sex is definitely a problem to him.

VIII 1. Moles are underground creatures. One quality is destructiveness and another is living in eternal darkness. This is like the turtle response. This person is underground, living within himself, hidden away from others. His request to read the back of the card indicates suspiciousness of the purpose of the cards. He fears being trapped by his responses and wants to stop the testing.

IX Slanting the card is an attempt to 'look into the picture,' that is, to get under the most prominent features of the card. It is another sign of fearfulness.

1. Coal is a symbol of life; it gives off energy and warmth. The source of warmth or life for this person has been extinguished. He is without love when it is needed most, that is, in the wintertime. There is an absence of life or love in all his remarks.

X 1. A tree stump is again something dead.

2. The sprig means a person about to be born. It is different from his other remarks that it may mean a fear of being born out of safety into a threatening situation. Is he a hospital patient that is about to be discharged? See how he comes back and makes it into a print, removing it from real life?!

Patient B's summary comments: This is a very sick person, but I could not say he was insane. I don't know. He is fearful, suspicious, guarded, lonely, sees himself as decaying or losing ground. He has many difficulties and burdens. He is very concerned about sex. Does he prefer other men or little girls? He is very withdrawn introverted, sees himself as actively burrowing, but I don't know whether he is going in or out, probably the first. He needs love very much but it is denied him for some reason and he feels lifeless and depressed.

Summary interpretation of the same Rorschach by Subject C:

This person has serious problems. He is very morbid, depressed, worries about life and death. He seems angry. Has he killed someone, a woman? Or does he want to be one? (Subject's interpretation of Card II: I think he means this is a female pelvis because the blood is misplaced. Does he enjoy hurting women? Or does he want to make himself into a woman?) I get the feeling that he is careful about what he says. He sees things he doesn't want to tell, but has to say something because he knows the doctor expects it. He is avoiding something that is very disagreeable; sex or violence, maybe both? The bat, turtle, and mole all have the same meaning: they are distasteful animals that hide themselves (in the night, water, earth). This person needs to hide himself from others. The last card shows his fear of death and the hope that he may live or grow again when things are better.

Comprehensive (summary) evaluation of the Rorschach respondent:

The patient, aged 31, diagnosed as "Schizophrenia, Paranoid type", has

been hospitalized intermittently the past five years. He is superficially pleasant and cooperative; however, he makes extensive employment of the defenses of repression, suppression, denial and projection.

His childhood was reportedly normal and uneventful except for a tendency toward seclusion. The first overt psychotic behavior appeared in the Navy. He makes insistent claim that an admiral made homosexual advances to him, which he rejected. In retaliation, the officer supposedly persecuted him during the remainder of his enlistment. The patient's marriage in 1948 apparently precipitated a homosexual panic, and shortly thereafter, he became acutely suspicious, fearful of being watched and harmed, and was hospitalized. While he was divorced in 1951, he nevertheless continues to blame his wife for all his present difficulties.

There is evidence of a rigid, crystalized delusional system. He believes his wife instigated his persecution by the Mafia and that this is perpetuated by his threat to expose the activities of this society. Personal difficulties are completely denied, hospitalization is perceived as imprisonment, and interviews as a form of interrogation. Hospital personnel are included as members of the persecutory pseudocommunity. For instance, he asserts that the ward physician is a member of the Mafia and each night administers "brain washings" while he sleeps. As evidence, he shows the "needle marks" on his body. He further believes that the hospital maintains unrelenting pressure in an attempt to cause his physical and mental deterioration. As is characteristic of many chronic psychotics, however, he has learned to avoid verbalizing his delusions. His outward conformance and submissiveness result from his conviction that present resistance is futile, and he has resigned himself to "taking it" until discharged.

Tenuous control over lightly suppressed, intense hostile and deviant sexual impulses is reinforced by his voluntary withdrawal from all interpersonal contacts, which he rationalizes as a protection of others, since father supposedly lost his job for trying to help him. This fear of contaminating others provides him with a convenient excuse for living away from his anxiety-arousing, controlling parents.

Aside from occasional episodes of verbal hostility, including remarks about the homosexual tendencies of other patients, the patient makes an adequate hospital adjustment. However, confrontation with any of the discrepant aspects of his delusional system or reports on irrational past behavior easily disequilibrate him and often result in heated counter-accusations. It is extremely doubtful if he can ever achieve other than a temporary marginal adjustment outside the hospital, especially since his defensive system will not allow him to accept sympathetic support and guidance. It is anticipated that the patient will someday require permanent commitment.

These are typical examples of the interpretive ability of the schizophrenic subjects, the degree of agreement between interpretations and between these interpretations and the case summaries. However, before clinicians scurry to hire schizophrenic consultants to aide them in Rorschach content analysis, they should be aware of two additional results. First, the subjects did not display equal facility in interpreting all Rorschachs, and second, there was definite evidence of personal projection in their interpretations.

An extensive psychological evaluation was available on each subject. When the five interpretations of any one subject were compared, they revealed clearly discernable trends consistent with their own dynamic structure. Patient A, for example, frequently found evidence of intense religious conflicts; patient B constant-

ly projected his own problems of homosexuality, including a preference for young girls; patient C, a convicted murderer, found many indications of homicidal impulses. That the subjects also displayed somewhat greater ability to interpret the protocols of schizophrenic patients than they did those of the neurotic or normal, seems partly attributable to a coincidental correspondence in the schizophrenic dynamics of the subjects and the patients whose Rorschachs they were asked to interpret. In addition, the possibility suggested itself from the data that the perceptional structuring of better adjusted individuals will be relatively impersonal, highly determined by the objective characteristics of the blots, and conflict-free, while the percepts of schizophrenics are more determined by impulses, defenses and conflicts. Records of this latter type appear to generally provide greater opportunity for interpretive elaboration in content analysis.

Discussion

There is suggestive evidence that some schizophrenics have a sensitive intuitive ability to understand unconscious imagery as projected in Rorschach responses, although the contribution of their projective mechanism must be carefully appraised. While primary interest here is in a novel technique for evaluating Rorschach content analysis, it is inevitable that the focus of discussion return to the nature of the schizophrenic thought processes. It is highly doubtful that the ability to interpret symbolism is specifically correlated with this disorder, since only three of fifty schizophrenics interviewed displayed this talent. There is evidence in the literature that some normal, psychologically unsophisticated hypnotic subjects who show no facility in handling symbolic content in the waking state are able to dream upon command or to interpret symbolism (5, 7). Freud (3, p.333), himself, believed that the presence of schizophrenia facilitates symbolic interpretation but in later life concluded: "The progressive experience of psychoanalysis has enabled us to discover patients who have displayed in a surprising degree this immediate understanding of dream symbolism. Many of these patients suffered from dementia praecox, so that for a time there was an inclination to suspect that all dreamers with such an understanding of symbols were suffering from that disorder. But this did not prove to be the case; it is simply a question of a personal gift or idiosyncracy without perceptible pathological significance." However, from a theoretical point of view, it is not surprising that the schizophrenic should have especial abilities in this area.

Schizophrenia is characterized by difficulties in communication; the schizophrenic lives in a symbolic world not shared to an appreciable degree by society. He has unusual difficulty in using shared, common, fixed, social symbols; instead, anxiety forces a system of logic that enables him to reason to acceptable, nonthreatening conclusions. This logic involves the basic principle elaborated by Don Doramus (10), and is in essence the false perception of identity based on irrelevant (predicate) attributes.

Thus the schizophrenic is bound to present experience, thinking and acting are directed by the immediate claims made by one particular aspect of the object or situation, as determined by intensely emotionalized attitudes. According to the specific way in which a certain object or situation is experienced, a restricted property or aspect becomes the basis for his choice of symbols. This is possibly the mode of schizophrenic thinking that contributes both to his difficulties in social communication and to his facility in understanding autistic symbolization.

The propensity for this mode of idiosyncratic thinking of the patient-

subjects was nicely demonstrated on Peters' (6) Word Meaning Test, a measure of abstract thinking. The subject is presented with a list of 30 stimulus words, each paired with four other words: One is subordinate, another supraordinate, and the other two are generally considered irrelevant. The subject is asked to identify which of the four "goes with" the single word. To illustrate:

Car:
- Regimentation
- Buick
- Paper
- Transportation

Shark:
- World
- Search
- Man-eating
- Animal

Month:
- January
- Finger
- Johnson
- Time

On this test the three subjects displayed a discernable tendency to associate logically irrelevant words;[1] for instance, Subject B paired Shark-World, Month-Finger, Car-Regimentation. Subsequent inquiry revealed that he perceived the world as full of people who, like sharks, are dangerous; he pictured a finger pointing at a calendar; he resents social regimentation and cars are one method by which this is achieved. However, when asked to choose the associations used by the "average person", the three subjects were quite capable of responding in the conventional manner. Thus the ' 'izophrenic is not invariably committed on this type of logic although its use has defensive value when a high degree of threat is involved.[2]

[1] While the subjects were distinguishable from a normal control group on this basis, many of the schizophrenics tested who displayed no special facility in symbolic interpretation also responded with idiosyncratic associations. The identifying characteristics of schizophrenics who show this ability remains to be determined.

Summary

Psychoanalytic theory posits that because of a breakdown of repression and the employment of archaic modes of thinking, the schizophrenic has especial faculty in understanding symbolic phenomena. A limited sample obviously restricts the conclusions that can be drawn from this exploratory study; nevertheless, it was found that in the absence of formal psychological training at least some schizophrenics have the ability to intuit personality dynamics manifested in Rorschach percepts. These results suggest an additional approach to the difficult problem of investigating the increasingly employed procedure of Rorschach content analysis.

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[2] This manner of reasoning is demonstrated by the situation in which Patient A once suggested that a diet of distilled water, refined food, and pure oxygen might be the cure for schizophrenia since the energy saved in the digestive process could be used to combat the illness. A mild objection by one of the staff was taken as confirmatory evidence by the patient on the ground that if the suggestion sounded "crazy" to a normal person and since insanity was a form of irrationality, then the remedy must be an illogical one.

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Overt and Covert Recording

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Gill, Newman, and Redlich (2) report that initial interview data were distorted and considerably abbreviated when recording was done by notes and from memory. Inasmuch as Thematic Apperception Test data may produce many thousands of words, depending upon the number of cards used, adequate recording of this test may become a problem of some concern. In the present age of electronics however, this problem is easily solved by the use of a recorder. This however raises another problem, for the presence of a microphone may by "mike fright" inhibit the production of useful material by the S or even be objected to by the tester—this latter is a facet of the problem which is often overlooked.[2] If it should be thought that the presence of the microphone may disturb the S and hence his productivity, the recording apparatus as well as the microphone may be concealed. At this point however, a hue and cry are raised that such a procedure is "unethical." In this connection Marcuse (3) states: "The very dynamics of the therapeutic situation involves the attempt to circumvent the patient's resistance and to wrest the meaning of the symptom from him. In this sense then all therapy is covert. To say that the procedure of covert recording which may help the patient is unethical, can only stem from a failure to recognize the dynamics of therapy." Distorted logic over "secret" recording has obscured beyond recognition the real issues involved—a point which Amrine and Sanford (1) have clearly indicated. In effect misuse is not a satisfactory reason for disuse. For purposes of this study the viewpoint of the American Psychological Association (7) is relevant: "Psychologists agree that clinical interviews, case records, and other aspects of diagnostic or therapeutic work should be kept confidential. They also endorse the general proposition that the client has the right to know the circumstances attendant on the conduct of the interviews. *However, specific decisions made in the interest of training, research, or some other laudable purposes are sometimes made in violation of these general agreements.*" (Section 2.31) (Italics the authors.)

Is it known whether overt recording actually does inhibit productivity? West (6) conducted a study in which he compared responses of 60 Ss in overt and covert situations. He found no significant differences in such indices as: number of words, reaction time, and clinical significance. However 15 per cent of his Ss, in their required phenomenological reports, did express discomfort at being overtly recorded. A possible explanation of his not finding significant results in certain of the indices is that his Ss were relatively well-adjusted and nonanxious individuals who were not influenced by the pressure of the presence of a microphone. It was the question of the role of anxiety in recording that prompted the present study.

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[1] This study was done in partial fulfillment of the requirements for the degree of Master of Science in Psychology at the State College of Washington. The scope of the study was made possible thanks to Grant 3220-218 by the State College of Washington.

[2] A recording of the interview implies a mirror that others besides himself can see and this may be why recording is not used. Concern may be more for the welfare of the therapist than that of the patient!

Hypothesis

The present study is concerned with differences in response to TAT cards of high and low anxiety Ss to overt (microphone and recording apparatus in view) and covert (microphone and recording apparatus concealed) procedures. It also asks the question as to whether, within the high or within the low anxiety groups, differences due to the type of recording, overt or covert, may be found. Finally it asks whether discomfort is present in the S during overt recording as revealed in phenomenological report.

Method

Since this study concerned the responses of anxious and non-anxious Ss, the problem of selection of such individuals had first to be considered. Inasmuch as the validity and reliability of the Taylor Manifest Anxiety Scale (4) has been demonstrated and it is at present frequently employed, this scale was used. All new students, 1,677 entering the State College of Washington in the Fall of 1955 were given the Taylor Manifest Anxiety Scale. These tests were scored for anxiety and lying. Ss with an "L" score of 7 and over ($N = 224$) were discarded. The upper 7 and the lower 7 per cent of the remaining male and female distribution were then selected. These comprised the sample of high and low anxiety Ss. The males had "A" scores either below 4 (mean $= 2.2$) or above 21 (mean $= 25.4$); while the females had "A" scores below 7 (mean $= 4.0$) or above 24 (mean $= 27.6$). The total number of Ss selected by this method was approximately 203 and of these 105 constituted the final sample. This final sample did not differ significantly from the 203 in any phase of anxiety. It consisted of 28 males high and 28 males low in anxiety while the female sample consisted of 29 high and 20 low in anxiety.

A total of eight TAT cards were administered to each S in two series of four cards each. These eight cards were chosen because they had been equated with respect to clinical significance (5). As a further precaution, they were also administered in an ABBA sequence. Series A comprised Cards I, IV, V, and X while Series B consisted of Cards II, IIIBM, IXBM, and XIV. Within each series the cards were always presented in a fixed order. Verbal instructions used in the administration of these cards were the standard ones.

In the covert recording situation the microphone was concealed in a filing box on the desk near the S while the recording apparatus was hidden inside a drawer. In the overt situation the microphone was near and obvious and the recording apparatus was placed in full view on the desk.

When the S knew that he was being recorded (overt situation) the E said: "It has been our policy to record some of these interviews. Do you mind if we use the recorder?" If the S objected (and three did), the E replied: "Using the recorder makes it much easier for me and the recording will not be heard by an unauthorized person." Beyond this point no one continued to or felt that he was in a position to object. If the recording was overt during the first, and covert during the second session, and the S were to ask, during the latter, whether the recorder was being used (no one did) he was to be told that it was not. The examiner in both situations sat in front of the S ostensibly taking notes in both the overt and covert situations.

The high and low anxiety Ss were each divided into four groups of approximately 28. Card series A or B were given approximately one week apart. The particular series of cards, A or B, and the overt (recorder obvious) and covert (recorder not obvious) situations were alternated for each group. The experimental design may be summarized as follows:

High Anxiety	Session I	Session II
Group 1	AO*	BC
Group 2	AC	BO
Group 3	BO	AC
Group 4	BC	AO

Low Anxiety	Session I	Session II
Group 1	AO	BC
Group 2	AC	BO
Group 3	BO	AC
Group 4	BC	AO

* Series *A* cards, Overt recording.

The factors analyzed were: seconds before response, word-count, rate (number of words per unit of time), clinical significance of series of stories (three clinicians rated the series as significant, indeterminate, or nonsignificant with respect to how much they revealed about the S), and a subjective (phenomenological) report. Agreement of all three judges was required in order to increase validity of the clinical significance criterion. In the phenomenological report the S was asked to check whether in giving stories when being recorded (overt) he found: less difficulty, more difficulty, or same difficulty than when (seemingly) not being recorded (covert).

RESULTS

Males and females high in anxiety and males and females low in anxiety were compared on the various criteria. In none of the criteria were the differences significant.[3]

In Table I, seconds before response, word-count, rate, and clinical significance of series of four stories are compared for differences between overt and covert recording for high and low anxiety Ss.

Analysis of overt and covert differences within high anxiety Ss shows a statistically significant different mean score for the first three criteria. All differences are in the same direction. That is, during overt recording high anxiety Ss respond faster, give a greater number of words, and talk at a faster rate than during covert recording. There is no meaningful difference in the ratings of clinical significance of series of stories between overt and covert recording. A similar analysis may also be seen in Table I for differences between overt and covert recording in low anxiety Ss. Here again we find Ss responding faster, giving more words (although not significantly greater) and talking at a faster rate to the overt situation. These differences are in the same direction as found with high anxiety Ss, but not as great. Again we find no meaningful difference in the ratings of clinical significance of series of stories between overt and covert recording.

In Table II, seconds before response, word-count, rate, and clinical significance, of series of four stories are compared for differences between high and low anxiety groups for overt and covert recording.

In none of the first three criteria (seconds before response, word-count, rate) were significant differences found in overt recording between high and low anxiety Ss. Difference between high and low anxiety ratings in clinical significance of series of stories appear, but are related to anxiety and not to the type of recording.[4]

Differences between high and low anxiety groups for overt recording were also analyzed. In none of the first three criteria were significant differences found. Difference between high and low anxiety ratings of clinical significance of series of stories appear, but again are related to anxiety and not to the type of recording.

Analysis of the phenomenological data show that thirteen of the high

[3] These data are available in "The Use of Overt and Covert Recording with the TAT" an unpublished thesis by Roger Edwin Sauer, State College of Washington Library, 1957.

[4] To test the possibility of an interaction effect between level of anxiety and type of recording, analysis of variance was done for three of the criteria. No interaction effect was found.

TABLE I—Mean Differences Between Overt and Covert Recording in High and Low Anxiety Subjects Per Series of Four Stories

Criterion	High anxiety (N = 56) Overt	Covert	t	Low anxiety (N = 49) Overt	Covert	t
Seconds before response	39.8±25.9	53.6±26.8	4.4*	46.7±37.2	59.8±45	2.4*
Word-count	380±221	336±181	1.9**	402±215	380±284	1.3
Rate	7.8±2.4	6.4±2.1	5.3*	7.9±3.1	6.9±2.9	2.1**
Significant series	8	6		3	1	

* p = <.01 ** p = <.05

TABLE II—Mean Differences Between High and Low Anxiety Subjects in Overt and Covert Recording Per Series of Four Stories

Criterion	Overt High Anxiety (N=56)	Overt Low Anxiety (N=49)	t*	Covert High Anxiety (N=56)	Covert Low Anxiety (N=49)	t*
Seconds before response	39.8±25.9	46.7±37.2	1.1	53.6±26.8	59.8±45	.8
Word-count	380±221	402±215	.5	336±181	380±284	.9
Rate	7.8±2.4	7.9±3.1	.3	6.4±2.1	6.9±2.9	.8
Significant series	8	3		6	1	

* p = insignificant in all cases.

anxiety and fourteen of the low anxiety Ss (a total of 26%) reported more difficulty in giving stories when being overtly recorded than when seemingly not being recorded (covert recording). Inasmuch as 20 of the 27 Ss (8 of the high and 12 of the low) reported more difficulty when overt recording occurred during the first session, it is necessary to bear in mind just when the recording occurs.

DISCUSSION AND CONCLUSION

Tables I and II show the presence of a difference related more to the method of recording (overt or covert) than to the presence of the degree of anxiety as measured by the Taylor Manifest Anxiety Scale. Overt recording was found to influence significantly high anxiety Ss to use a greater number of words, to respond more quickly, and to talk at a faster rate than during covert recording (Table I). This was true (though not with word-count) to a lesser degree in low anxiety Ss.

The lower word output, slower rate of speech and possibly the slower reaction time to covert recording may result from a tendency on the part of the S to pace the writing speed of the E. While the data do not permit a definitive answer, certain considerations make the assumption of pacing rather dubious. The physical set-up employed did not permit the S to obtain a clear view of the E's writing speed. Secondly, the psychological situation involved naive freshmen taking a personality test in which it might be assumed that they would be more concerned with their own feelings than with the question of pacing. Finally, although not bearing on either side of the question, it may be remarked that the situation encountered by the S is the standard clinical one.

Although differences were found, the importance of these differences is open to question in light of the lack of results found for the clinical significance of stories (Table I). Possibly the ratings of clinical significance may be questioned. Most important of all the findings, though again not without qualification, is the fact that over one-fourth of the Ss reported greater difficulty (phenomenological data) when being overtly rather than being covertly recorded. So on the one hand some might argue that the phenomenological data contraindicate the use of overt recording, while others, pointing to the results found with the first three criteria (Table I), might argue exactly the opposite. The criterion of clinical significance of stories which might help in solving this dilemma remains mute. It is one thing to find a difference and another to know exactly what such a difference indicates. It could at least be argued that first interview overt recording of the TAT is probably unwise.

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"Diagnosing" Szondi's Pictures

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The literature contains a number of studies dealing with the ability of subjects of various degrees of psychological sophistication to "diagnose" Szondi's pictures, that is, to match the stimuli of the Szondi Test (8) with their proper psychiatric diagnoses. Best and Szollosi (1), Fosberg (3), Holt (cited by Deri (2)), and Rabin (5, 6) all report that their subjects were more successful at this task than chance alone would allow. The aim of the present investigation was to determine whether performance in the psychodiagnostic matching procedure is associated with certain personality dynamics of the "diagnostician." Specifically, it was predicted that the frequency and the accuracy with which a subject used a given diagnostic category in this procedure would be related to the strength of the drive represented by that category, and the subject's acceptance of the drive, as measured by the Szondi itself. These predictions were based on the assumption that such processes as sensitization, vigilance, and perceptual defense might be operative in psychodiagnostic matching, as they have been shown to be in a variety of other experimental situations.

SUBJECTS AND PROCEDURE

The subjects were 20 male undergraduate college students majoring in psychology. All had completed a course in abnormal psychology, and none of them was familiar with the Szondi. Each of the subjects was examined individually, starting with a standard administration of the test. Immediately afterward, the subject was told that the stimulus pictures were photographs of psychiatric patients. He was presented with a list of the eight diagnostic categories which appear in the test and instructed to match the pictures, one by one, with the diagnoses. The subject was not advised that there was an equal number of pictures for each category.

The test was scored in the manner prescribed by Szondi (9). Two measures were obtained for each of the eight test factors: a "quantitative" score, based on the absolute number of selections made from the six pictures of a factor; and a "tendency" score, based on the distribution of "likes" and "dislikes" within the factor. In interpreting the test, it is assumed that the first of these scores reflects drive strength, and the second, the subject's acceptance or non-acceptance of the drive.

RESULTS AND DISCUSSION

The distribution of accuracy scores in the psychodiagnostic matching procedure is shown in Table I. By chance, the subjects would have been expected to make an average of six correct matches out of a total of 48 (4). Actually, only one subject made fewer than this number, and the group median was 11. Four of the 20 subjects differed significantly from chance at the .05 level (11 or 12 correct matches) and four more at the .01 level (13 or 14 correct matches). The number of significant differences at both levels is itself significant at the .001 level (7). These findings corroborate those of previous investigators.

To test the predictions offered above, distributions of the frequency and accuracy scores for each factor were dichotomized as near the median as possible, and chi squares were computed between these scores on the one

[1] Formerly at New York University, where the data for this study were collected.

TABLE I—Distribution of Accuracy Scores

Correct Matches	f
5-6	1
7-8	5
9-10	3
11-12*	7
13-14**	4
N	20

* Significance is a function not only of the number of correct matches, but also of the number of times the subject uses the various diagnostic categories. For this reason, only four of the seven subjects in this class differed significantly from chance at the .05 level.

** Significant at the .01 level.

hand and the Szondi "quantitative" and "tendency" scores on the other. Table II gives the results of this analysis. Only one of the 32 chi squares is significant at the .05 level. In short, the data offer virtually no support to the research predictions.

On the basis of the available material, it cannot be determined whether the negative results are due to the misapplication of a need-in-perception frame of reference to psychodiagnostic matching, or to the shortcomings of the Szondi as a predictor. While the findings of Best and Szollosi (1) conflict with those of Rabin (5, 6) on this point, it may well be that the ability of an individual to "diagnose" accurately Szondi's pictures is simply a function of his familiarity with the diagnostic categories in question, and is not related to his own personality dynamics.

TABLE II—Relationships between Psychodiagnostic Matching Scores and Szondi Scores, Tested by Chi Square

	Szondi "Quantitative" Score[1]		Szondi "Tendency" Score[2]	
	Frequency Score	Accuracy Score	Frequency Score	Accuracy Score
Homosexual	1.010	0.785	1.122	4.826
Sadist	0.069	2.500	2.639	4.097
Epileptic	2.081	1.000	0.729	4.667
Hysteric	2.054	5.065	0.683	3.234
Catatonic Schizophrenic	6.117	2.079	5.020	3.882
Paranoid Schizophrenic	2.443	0.144	0.254	2.972
Manic Depressive, Depressed	2.812	2.812	5.020	1.436
Manic Depressive, Manic	1.000	4.896	3.077	3.253

[1] Critical value at the .05 level for 2 d.f. = 5.991.
[2] Critical value at the .05 level for 3 d.f. = 7.815.

SUMMARY AND CONCLUSIONS

The Szondi Test was administered to 20 undergraduate psychology majors, who then attempted to match the stimulus pictures with their proper psychiatric diagnoses.

1. Four subjects differed from chance at the .05 level and four more at the .01 level in the number of correct matches made. The number of significant differences is itself significant at the .001 level.

2. The frequency and the accuracy with which a subject used a given diagnostic category was related neither to the strength of the drive represented by that category nor to the subject's acceptance of the drive, as measured by the Szondi.

It would appear that performance in the psychodiagnostic matching procedure is not a function of the personality dynamics of the "diagnostician." Evidence from other studies suggests that familiarity with the psychiatric disorders represented in the Szondi may be the critical factor in making accurate "diagnoses."

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Productivity and the Clinical Use of TAT Cards[1]

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A previous paper (9) introduced a method for scoring the number of emotional words produced by subjects in response to TAT pictures, and used this measure of material as a piece of information which has validity in relation to a criterion and yet can be related to global judgments by clinicians of the criterion. The present study deals with some of the problems touched on in the first investigation. The questions are: 1. What is the relationship between the clinical use of TAT stimuli and the amount of material that is likely to be produced in response to the stimuli? and 2. Can the measure of material produced be used as a dependent variable in attempts to specify the characteristics of stimuli as they affect the subject?

This study takes as its first hypothesis that some TAT cards elicit more material than others. In operational terms: there are significant differences in the median number of emotional words per story for different TAT cards. If the first hypothesis is accepted, a second can be advanced, i.e., that clinicians tend to select cards which elicit more material. In operational terms: cards eliciting a higher median number of emotional words per story are more frequently used than are cards producing a lower median number of emotional words.

If the first two hypotheses can be accepted, a third may be advanced to the effect that a positive relationship exists between the measures used in the first two hypotheses and other measures of TAT productivity. To test this hypothesis, data reported by Eron (2) will be used. If this third hypothesis is accepted, a group of related measures of amount of material produced in response to TAT stimuli will be available to investigate characteristics of stimuli as they may effect productivity. To do this, scores used in the third hypothesis will be compared with information reported by Bijou and Kenny in a series of papers (1, 5, 6).

METHOD

Sample:

The sample consists of the five most recent TAT protocols administered by each of 35 clinical psychology trainees at a VA neuropsychiatric hospital. All of the 175 protocols were of hospitalized males between the ages of 19 and 59, who had been referred for diagnostic evaluation, and who dictated their responses to the examiner. Selection of the cards, instructions, inquiry and stenographic skill varied with the examiners. The examiner group had all had at least one year of graduate school and some experience with tests before starting their training at the hospital. All had had or were taking university sequences on the theory and application of tests. These examiners were not naive but were consciously trying out and learning about tests. Added to the variability due to previous training and experience were the frequent pressures of time and subjects' limited ability to cooperate. There were 24 male and 11 female examiners. There were no significant differences between these two groups on the scores used in this study.

Scoring:

The method for scoring number of emotional words per TAT card re-

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sponse is that introduced by Ullmann and McFarland (9). This method was shown to have a rater reliability for 20 homogeneous six card protocols of .92 product moment and .94 rank order correlations. The score of frequency of use of TAT cards is the number of times each card was used in 175 protocols.

Cards:

The stimulus material reported on in this study was the set of adult male cards as designated by Murray (7). These 20 cards comprised 93.5% of all card responses in the present sample. The use of an adult male population, the desire to compare findings with other studies, and the small N in the present sample of scores for cards not in the adult male set, led to the limitation of the present report to the 20 adult male cards.

Results

The first hypothesis is that there are differences in the median number of emotional words elicited by TAT cards. The 20 by 2 table of 20 cards, high or low median number of emotional words, yields a chi-square of 143.39, where for 19 degrees of freedom, 43.82 is significant at the .001 level of significance. More powerful statistical tests would merely serve to accentuate the reliability of the differences between cards. Thus, there are clear differences between cards in terms of the number of emotional words elicited, and the first hypothesis may be accepted.

The second hypothesis is that the median number of emotional words elicited per response to the TAT cards is positively related to the frequency of use of these cards by clinicians. Table I presents the cards, the frequency of use, the number of examiners using the cards, and the median number of emotional words elicited per card response. The relationship between frequency of use and median number of emotional words yields a tau of .62, which for N of 20 is significant beyond the .001 level of statistical significance (8).[2] Thus, the cards eliciting the greatest number of emotional words are used more frequently, and hypothesis 2 may be accepted.

Table I. Cards, Frequency of Use (FrU), Number of Examiners Using (#Ex), and Median Number of Emotional Words Per Card (Mdn.EW), for 175 Protocols.

Card	FrU	#Ex	Mdn.EW
1	153	34	5.50
2	104	30	4.77
3	147	34	5.64
4	155	34	6.56
5	72	28	3.50
6	157	34	7.63
7	142	33	5.81
8	91	30	4.42
9	46	23	2.50
10	68	26	3.70
11	33	18	2.80
12	122	33	4.09
13	154	35	5.37
14	70	25	3.17
15	34	18	4.50
16	41	17	3.50
17	69	26	3.08
18	88	30	5.31
19	18	10	2.88
20	48	22	4.38

Eron presents data as to the number of thema elicited per TAT card (2, p. 25, Table 14) and the variety of thema elicited by each card (2, pp. 37-46, sections D). The score used in the present study for variety of thema is the number of different thema per card with 5% or greater frequency, for Eron's 75 hospitalized males. Data bearing on the expectation that the measures of productivity and use pre-

[2] Heppell and Raimy, using institutionalized boys as subjects, note that "The analysis of variance showed that the differences between picture means (based on summating ratings of all four judges using *both* rating scales) could not be accounted for by the unreliability of the judges." (4, p. 409). Heppell and Raimy present the rank order of 15 TAT cards as to usefulness in understanding child-parent relationships (4, p. 409, Table 1). For these 15 cards, a tau of .33 is obtained with the median number of emotional words and a tau of .53 with frequency of use, the latter being significant at the .05 level.

sented in this investigation are positively related to other data reported in the literature are presented in Table II. Table II shows the interrelationships by tau between the number of thema reported by Eron (E-#T), variety of thema reported by Eron (EvT), frequency of use as reported in this study (FrU), and median number of emotional words elicited per card as reported in this study (MdnEW). The expectation of positive relationships between data reported by Eron and data reported in this study, as to productivity of TAT cards, is substantiated; all the interrelationships in Table II are statistically significant at the .05 level or beyond.

TABLE II. Interrelationships of Measures of Productivity from Eron (2) and as Found in This Study. Relationships Are Tau Based on 20 TAT Cards.

	E#T	EvT	FrU	Mdn.EW
E#T	.. ..	.56	.33	.41
EvT		.. ..	.34	.34
FrU			.. ..	.62
Mdn.EW				.. ..

Significance of tau with 20 cases is: .32 at the .05; .41 at the .01; .54 at the .001, two tailed test.

Bijou and Kenny (1) rank-ordered TAT cards as to ambiguity on the basis of judgments by college students. Kenny and Bijou (6) presented data to demonstrate that level of ambiguity is curvilinearly related to amount of personal or emotional meaning injected by subjects into stories, with cards of intermediate ambiguity yielding the most material. In these studies, cards 5, 15, 18BM, 11 and 19 are designated as the most ambiguous set, cards 4, 7BM, 8BM, 12M and 13MF are designated as the intermediate ambiguity set, and cards 12BG, 1, 2, 9BM and 17BM comprise the least ambiguous set. Since the projection of personal or emotional themes is the sort of grist clinicians desire, frequency of usage should be greatest for the intermediate ambiguity group. Kenny (5) also demonstrated that transcendence is greatest for the intermediate ambiguity group. Since a larger number and variety of thema and number of emotional words should be associated with higher transcendence, these scores should be greatest for the intermediate ambiguity group. Data in this and Eron's studies are available for 14 of the 15 cards (all but 12BG) used in the series of studies by Bijou and Kenny.

Applying Kruskal-Wallis one-way analysis of variance (8), the expectation that clinicians make most frequent use of cards in the intermediate ambiguity group is substantiated between the .05 and .02 levels of statistical significance. Cards of intermediate ambiguity elicit the most material (number of thema, variety of thema, number of emotional words). However, the relationships between levels of ambiguity and number of thema and number of emotional words by application of the Kruskal-Wallis test are at the .20 level of statistical significance. The relationship between levels of ambiguity and variety of thema is totally insignificant.[3]

DISCUSSION

The present study uses the method, introduced by Ullmann and McFarland (9), of quantifying emotional words on the TAT to obtain data on the relationship of elicited material to clinical use. As in the previous study, the clinician behaves in a reasonable manner. In the present case, clinical psychology trainees more frequently use the TAT cards that are likely to give them the most data. While the rationality of clinical psychology trainees is worth noting, it is

[3] It is not the purpose of this paper either to confirm or to question Kenny's findings. The results are presented to illustrate a method, whether degree of ambiguity or some other variable, such as the frequency of pictures of two people interacting, is the attribute under investigation.

possible to wonder whether this group of examiners would have explained their choice of TAT cards on the basis of increasing the data with which they would work, or whether they would have used theories that make assumptions about the personalities of particular patients. To accept the latter explanation, it would be necessary for the examiners to demonstrate that their reasons add to the basal relationship existing between frequency of choice and the amount of material elicited. Both studies suggest that it is possible to devise relatively simple descriptions of the clinician's role, and in two particular cases, these formulations are significantly related to the clinician's behavior.

For future research, two facts seem worth pointing out. As Heppell and Raimy (4, p. 409) succinctly put it, " . . . in a given series of similar pictures some will, generally, prove more useful than others." The present study yields data which point to differences of considerable stability between TAT pictures. The data of Table I may supplement Eron's normative work and have use in the selection, equation or ordering of TAT stimuli for research.

A second aspect of this study which has bearing on future research is the demonstration with information from the studies of Bijou and Kenny that it is possible to devise a measure of subjects' responses to TAT and utilize this measure when aspects of the stimulus are varied. While great ingenuity has been demonstrated in the development of Q sorts, ratings, etc., as measures of the effect of changing the stimulus, it seems that a single acceptable method of measuring the effect on subjects' responses will conserve energy and increase the number of directly comparable studies.

Summary

The purpose of this research was: (1) to investigate differences between TAT cards as stimuli leading to the production of emotional words; (2) to relate these differences to the frequency of use of the cards in a clinical setting; (3) to compare these results with other reports of productivity of response to TAT cards; and (4) to demonstrate the practicality of using scores such as the median number of emotional words as a measure of the effect of stimulus variables on the responses of subjects.

From each of 35 clinical psychology trainees, five protocols obtained as part of routine diagnostic evaluation were selected. All the 175 protocols were of males, hospitalized for neuropsychiatric reasons, who dictated their responses to the examiner. The protocols were scored for number of emotional words for each card by the method introduced by Ullmann and McFarland (9).

Highly reliable differences were found between the median number of emotional words elicited by different TAT cards. The frequency with which the cards were selected for use by the examiners was related positively and significantly to the amount of material elicited by these cards. The findings with respect to "card pull" and frequency of use are in general agreement with previous findings reported by Eron (2), Heppell and Raimy (4), Kenny (5) and Kenny and Bijou (6).

The study is discussed in terms of a description of an aspect of the clinician's role and in terms of future research with the TAT.

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Responses of Chronic Schizophrenic Patients to Tachistoscopic Presentation of Rorschach Figures

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INTRODUCTION

Research in psychiatric illness has in recent years been more commonly oriented toward an understanding of the dynamic and genetic factors involved than toward an understanding of the actual physiological and psychological processes utilized. The development of a delusional system has been extensively studied and interpreted through the application of psychoanalytic techniques. An understanding of the sensory and perceptual mechanisms operative to make a delusion possible is much less clearly understood. It is our belief that an understanding of "how" a delusional system develops warrants considerable study. Reading of the current literature and non-systematic referral to the older literature offers a number of leads, but nowhere is there a clear picture of how a delusion is initiated and developed.

A delusion implies that there is a reception of different data from the environment or a different evaluation of sensory data than is held by most people. The latter explanation is consistent with an interpretation based on psychodynamic factors, on parataxic distortions. We are not certain that all such distortions are psychodynamically based. Theoretical formulations on other bases are certainly justified, and continued investigations from varied viewpoints are essential.

A concept fairly widespread, and to our mind quite valid, is Sullivan's concept of schizophrenic empathy. According to this concept, the child destined to potential schizophrenia learns early to sense and respond to anxieties and hostilities of the parent. These responses frequently arise in conjunction with overt parental behavior of a contradictory nature. The child picks up minimal stimuli, frequently of a "first impression character," and holds to the evaluation thus formed. Again, it is frequently stated that one of the reasons for the difficulty so often encountered in psychotherapeutic efforts with schizophrenics is that such patients readily pick up the therapist's controlled anxieties, resentments, and hostilities, and more importantly, the schizophrenic is able to make an extremely astute observation about the therapist's weaknesses and to exploit them in his relationship with the therapist.

These clinical observations tend to indicate a rapid but faulty evaluation of certin aspects of reality. The patient appears to be unable to correctly assess the relative value of contradictory evidence. He is unable to correct himself on the basis of additional data.

One notes the rapidity with which certain "normal" people form erroneous impressions of the environment, completely out of keeping with actuality, and then elaborate a fairly complete picture on the basis of this perception. A very simple example of such behavior follows: An individual riding down the street in a strange town, unable to see all of the sights, will frequently incorrectly read a sign over a place of business, and from this very insufficient evidence, elaborate the data necessary to form an impression or Gestalt and then proceed to

discuss this elaboration as if it were correctly sensed and perceived. This sort of phenomenon, although similar to the confabulatory Rorschach response, is not always a sign of psychosis. It occurs in individuals who function adequately in most areas and who are quite open to correction of such erroneous initial impressions. It is at the point of their willingness or ability to admit error that they differ clearly from the schizophrenic. Another example of this relatively common phenomenon is the tendency of many hard of hearing persons to attempt to fill in the data that they miss. In these last situations, we find a tendency to complete the Gestalt and a willingness to change the original impression, if necessary. In all of these, there appears to be primary reliance upon initial impression.

From this purely clinical observation, we arrive at the following questions about the schizophrenic:

(1) Is it possible that a characteristic of the schizophrenic is a tendency to form impressions on such minimal data and to continue to hold such evaluations in spite of opportunity to correct these initial impressions by the addition of further data?

(2) Is there any method by which we can quantitatively study the formation of concepts from stimuli in the schizophrenic and the modification or preservation of such concepts in the face of quantitatively greater data?

It occurred to us that a very good method of testing would be to devise some test situation in which stimuli could be readily varied, either by time or intensity, and to study patients in that situation. Suggestions of such an experiment were obtained from the research studies of Douglas (1), Stein (2), and Weisskopf (3). It was felt that if such study were undertaken and it appeared to warrant further investigation, it would be of great advantage if the technique used were one that could be readily reproduced.

A number of test situations were considered. The one that appeared to offer the most advantages was the use of Rorschach slides projected at varying speeds, with the plan to add other test situations later.

It is our hope in this study to follow over a relatively long period of time a relatively small group of patients, whom we have selected from the continuous care service of a psychiatric hospital. By this method we hope to find possible correlations between patients' symptoms, their behavior in the hospital and in the various test situations that are planned. It is felt that it may be a possible lead to greater understanding of the actual physiological and psychological processes associated with schizophrenia.

PROJECT PLAN

The objective was to study the responses of schizophrenic patients to a limited variety of stimuli, in this case, Rorschach figures projected upon a screen for variable short periods of time. The responses of patients to a standard image exposed to view for progressively longer periods of time were to be assessed in an attempt to ascertain whether or not there appeared to be any definite pattern of response or possibly more than one definite pattern. These responses could be studied as phenomena which would be valid for the group of patients being studied, as well as for larger groups, and no attempt to discover in them a definite pattern, unique to schizophrenia, was intended.

SUBJECTS

The subjects investigated were sixteen chronic schizophrenic patients selected randomly from a chronic ward of the continuous treatment service of a Veterans Administration psychiatric hospital. The selection of chronic schizophrenics was made for the following reasons:

1. The examiners were interested in working with schizophrenics, the largest clinical group in the hospital.
2. The chronic group would include more patients not previously exposed to the Rorschach.
3. The problem of statistical treatment of data could be more easily handled in a homogeneous group.
4. The same subjects could be used for later studies with other test situations, the risk of their being discharged and unavailable at a later time being less.

Apparatus

The apparatus and materials used in this study included the following:

1. Slide projector (35mm).
2. Tachistoscopic shutter for slide projector so exposure time could be regulated.
3. Set of trial demonstration slides made locally.
4. Set of standard 35mm. Kodachrome Rorschach slides.
5. Screen.
6. Recorder — for getting a record of patient's responses.
7. Stop watch used for exposures longer than one second.
8. Paper, pencils, etc.
9. Adequate shades for darkening the experimental room.

Personnel

The following personnel were used in securing data:

1. Test examiner, who was a person trained to give the Rorschach test.
2. Projectionist, who projected the slides and controlled time of exposures.
3. Recorder, who obtained a written and a disc recording of responses given.[1]

Method and Procedures

Subject was seated at table directly in front of center of screen and at a distance such that the image on the screen subtended an arc, the same angle as that which would be made by the card held in the subject's hand at the usual distance. This calculation standardized the response as far as relative size of stimulus was concerned and corresponded, in relative size, to the same image in the usual situation. The room was darkened adequately for projection purposes. The test examiner sat to the left of the subject, the recorder to the right and somewhat behind the subject, and the projectionist immediately behind the subject.

All subjects had the test procedure explained to them. An explanation of the figures, similar to that given in the usual Rorschach test, was provided. In addition, they were told that the image would be seen for only an exceedingly short time. The subjects were then instructed that trial slides would be projected as examples of what the test was about. These inkblot figures were prepared locally. The demonstration slides were then projected and the responses called for and recorded as they were to be when the Rorschach slides were projected.

If the demonstration indicated that the subject was not responding adequately, he was questioned as to whether he understood, and any uncertainty was cleared up. If, after further explanation, the response was still inadequate, it was assumed that the cooperation of the patient was not being obtained and patient was not a suitable subject for the experiment. If the demonstration indicated that the patient was able to cooperate, he was then carried through the Rorschach procedure.

The Rorschach slides were placed in an automatic slide changer to insure that in the test there would be no chance of reversing the slides. They were presented in the usual sequence of 1-10. The subject was asked to respond to each projection. The first projection was for a period

[1] Louis W. Bova, Jr., clinical psychology graduate student, University of Florida, served as recorder; the authors wish to express their appreciation to him for his assistance.

of 1/100 second. After all ten slides had been projected, the series was repeated at 1/10 second, then at 1 second, and finally for an unlimited time. During the procedure written and disc verbatim recordings of the responses were made and this record transcribed for study, the dual recording being made to insure accurate records of responses as given.

For purposes of comparison, a complete Rorschach record was secured about two weeks later, this record being obtained by administration of the Rorschach in the usual way. Two of the subjects were not available for the regular Rorschach testing, because they had been transferred to another hospital.

RESULTS AND DISCUSSION

In Table I, the number of responses for each subject is given for each presentation of the Rorschach figures with time of exposure varying from 1/100", 1/10", 1", to unlimited time and the regular administration of the Rorschach cards.

It may be seen that there is little difference in the number of responses obtained from the 1/100", 1/10", and the 1" exposure of the figures. When there was unlimited time given, the number of responses was about double of the number obtained for 1/100". The increase in number, however, is not as great as one would expect with unlimited time given for observation of the figures. Many of the subjects, it may be noted, gave as many for the short time presentation as for the unlimited time. It is significant to note that there was little difference between the unlimited time presentation of the figures on the screen and the presentation of the figures on the cards as is done in the regular administration of the Rorschach test. While time of exposure of the figures was markedly varied in the five series of presentations or trials, it may be observed that for many subjects it was not a primary factor in determining number of responses given.

When the examiners were obtaining the data, they noted the marked tendency for the same response to be given by a subject even though the figures were different. Table II shows the frequency with which this tendency manifested itself. The first number for each trial is the number of times a given response was given by that subject during the presentation of the Rorschach figures in that particular trial with given time exposure. If the repetition or perseveration was found using a second repeated response, such is indicated in the table

TABLE I—Number of Responses for Varying Time of Exposure

Trial or Series	1	2	3	4	5
Subject No.	1/100"	1/10"	1"	Unlimited T	Regular Admn.
1	8	10	10	10	12
2	10	10	8	22	18
3	0	0	0	6	10
4	6	4	7	15	27
5	13	10	10	10	18
6	7	5	5	8	3
7	8	10	10	18	18
8	10	11	8	13	12
9	5	2	4	14	....
10	8	7	9	10	....
11	7	10	10	18	10
12	8	10	11	12	16
13	8	5	7	14	10
14	2	1	5	4	11
15	5	5	8	7	11
16	1	5	3	10	10
Average R	6.3	6.3	7.2	12	13

by using commas between the repeated responses. The number of repeated responses for each subject for each series is given in the numerator; the total responses given for the trial by the subject is given in the denominator. The percentage of responses repeated is then given in the immediate column to the right. The average percent for each trial or series is then given in the lowest or last horizontal line of the table.

It may be seen that for all trials, including those providing unlimited time and those involving regular administration of the figures on cards, the frequency of repetition of the same response was marked. In several individual cases, perseveration was found in 80 to 100% of the responses given. When overall averages were computed for the subject group as a whole, it was seen that primary perseveration occurred in from 30 to 36% of the responses, while secondary perseveration was found in an additional 7 to 13% of the responses. For the group as a whole, from one-third

TABLE II—Table Showing Number of Repetitions of Same Response, Perseveration (P), in Ratio to Total Responses (R) Given

	1/100"		1/10"		1"		Unlimited T		Regular Admn.	
	P		P		P		P		P	
Subject	R	%	R	%	R	%	R	%	R	%
1	2, 2	25, 25	2, 2, 2	20, 20, 20	3	30	3	30	3, 2	25, 16
	8		10		10		10		12	
2	3, 2	30, 20	2	20	2	25	3	14		
	10		10		8		22			
3	0		0		0		6	100	6, 3	60, 30
							6		10	
4	2	33					3	20	3	11
	6		4		7		15		27	
5	3, 2	22, 15	3, 2	30, 20	3	30	4, 3	40, 30	6, 5	33, 30
	13		10		10		10		18	
6					2	40	2	25		
	7		5		5		8		3	
7	2, 2	25, 25	2, 3	20, 30	2, 2	20, 20	6, 3	33, 16	6, 2	33, 11
	8		10		10		18		18	
8	3	30	3, 3	27, 27	3	37	3, 2	22, 15	2, 2	16, 16
	10		11		8		13		12	
9	2	40	2	100	0	0	2	14		
	5		2		4		14			
10	6	75	6	86	4, 2	44, 22	5, 3	50, 30		
	8		7		9		10			
11	5	72	8	80	8, 2	80, 20	6	33	8	80
	7		10		10		18		10	
12	2	25	2	20	2	18	2	16	6, 2, 2	37, 12, 12
	8		10		11		12		16	
13	2, 2, 2	25, 25, 25	2	40	4	57	5, 2	36, 14	10	100
	8		5		7		14		10	
14					2	40			2, 2	18, 18
					5				11	
15	2	40			3, 2	37, 25	3	43	2, 4	18, 36
	5				8		7		11	
16							8	80	7	70
							10		10	
Average % P		30, 10		30, 7		30, 7		35, 7		36, 13

to one-half of the responses were repetitious or perseverative in nature.

A study of the quality of the responses revealed a definite concretization of thought and loss of abstractive capacity. Even with extended time and in the responses given during the regular administration of the Rorschach, the responses yielded were simple concrete wholes with little organizational or integrative quality.

The high frequency of perseveration may be tied in or related with the number of responses as given in Table I. It appeared as though the chronic schizophrenic tended to have a "one track mind." He tended to see one thing, and that was about all. This perseverative tendency reduced the number of responses given, and it made little difference whether the subject saw the blot for a very limited time or whether he had unlimited time to observe it. Most stimuli from the environment appeared to be viewed or interpreted from a set, preexisting frame of reference, and variation in the objective stimuli produced little effect in images perceived and reported.

Whether the perceptual frame of reference was determined by psychological factors, such as effects of earlier experiences, or by physiological factors, such as neurological or biochemical condition of the cerebral cortex, was not apparent from data of this study. Further research will be needed to elaborate the determinants of this perceptual rigidity.

We plan further studies to determine whether this perseverative tendency is exhibited in the same group of subjects when subjected to other test situations. We also expect to use the same procedure described above with other diagnostic groups of psychiatric patients and a non-patient group to determine whether other groups respond with any different patterns of perceptual behavior.

CONCLUSIONS

1. When chronic schizophrenic subjects were presented projected Rorschach figures for varying exposure periods ranging from 1/100th second to unlimited time, there was little increase in the number of responses given to longer as compared to very short exposure periods of time.
2. Even when unlimited time of viewing the figures was provided, the number of responses was small, the usual number being about one response per figure.
3. There was considerable repetition of the same response or perseveration, this averaging one-third to one-half · when computed for the subjects as a whole; some individual subjects showed perseveration in 80-100% of their responses for given trials.
4. Chronic schizophrenic subjects revealed an inability to shift or alter their frame of reference so as to perceive real differences in environmental stimuli, resulting in marked perceptual rigidity and inflexibility, which appeared to be characteristic of this disorder.

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A Note on the Generality of Constriction[1]

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PROBLEM

The use of the term constriction is prevalent in clinical psychology, although little logical or psychological analysis of its meaning appears in the literature. Within most contexts there is the implicit assumption that constriction is a general personality characteristic which can be identified in a variety of different situations. There is, however, virtually no evidence either to support or refute this hypothesis. Carp (3) found that although constricted behavior could be rated reliably by clinical psychologists on any one of three measures, there was no agreement between measures. Klein (4), working within the framework of psychoanalytic ego psychology, inferred a general personality dimension which he termed one of constrictive-flexible control. The study reported here represents an attempt to clarify the meaning, or meanings, of this concept.

In order to examine the generality of constriction, an analysis first was made of the contexts in which the concept appears. From this analysis three uses of constriction in clinical psychology emerged.

1. It is used to characterize certain kinds of performances on commonly used clinical techniques for appraising personality functioning.
2. It is used to refer to a restriction or inhibition of certain behavioral characteristics.
3. In a more impressionistic way it is used as a gross descriptive term applied to an individual.

[1] This report is based on a dissertation submitted to the Department of Psychology and The Graduate College of the University of Nebraska. I wish to express my gratitude to Dr. D. W. Dysinger for his help in supervising the research.

METHOD

On the basis of this analysis, measures were selected and created to exemplify these various usages. Methodologically, this can be considered a study in "construct validity" (1). None of the measures used can be considered a definitive criterion of constriction but all of them are related to the concept of constriction as it is used by clinical psychologists in the above mentioned categories.

The first measures to be described were selected on the basis that they are commonly used clinical techniques in connection with which certain performances are designated as constricted. The only one of these for which the literature clearly indicates criteria for constriction, is the Rorschach test. Most contributors to this literature emphasize that an overabundance of accurate, form-determined responses is the basic definition of constriction on this instrument (2, 5, 6, 9, 11). In addition, some writers specify that stereotypic thinking as indicated by the percentage of animal percepts in the content of the record and a paucity of responses are effects of the constrictive process. In terms of Beck's (2) scoring categories for the Rorschach techniques, the constriction indicators are: percentage of form-determined responses (F%), quality of form-determined responses (F+%), animal percent (A%), and number of responses (R). In order to deal with the overall record, all of these categories were combined to derive an index of constriction by converting the score on each category into a standard score and summing these standard scores.

Another commonly used clinical device on which performances are commonly termed constricted is the TAT,

although this term is not found regularly in the TAT literature. An exception to this is the work of Rapaport (11), who indicates brevity of the stories and a meticulous clinging to description as indicators of constriction. A rationale for an index of constriction on this technique may be derived from personality theory. An individual who is guarded and not free to express himself unless the situation is well defined will give brief descriptive stories rather than long and more involved ones. Thus one measure would be length of the story, while another might be complexity of the theme created. A high correlation has been found between productivity as measured by the number of words used, and a theme complexity variable (10). It was decided, therefore, to use brevity of the stories as indicated by the number of words used as the TAT index of constriction for this study. The mean number of words per card was the constriction score for each subject. Six cards of the TAT were selected for the study on the assumption that they would sample adequately the variable being measured. The cards employed, in the order of administration, were 3BM, 5, 14, 15, 20, 16.

The third clinical test used was the Bender-Gestalt test. In general clinical usage, primary indicators of constriction are the area of the paper used and the size of the designs. The measure selected for this study was size of the designs with the assumption that this would be correlated with area used. Measurements were made by constructing a grid on a transparent piece of plastic, laying it over the design, and counting the number of grid squares covering each design. The score was the sum of the grid squares occupied by all of the designs for each subject.

The next two measures were selected to represent usages of the concept where it is defined as inhibition or narrowness in interests and general responsiveness. As a measure of the extent to which a person can invest himself in interests, an Intensity of Values Test (12) was utilized. This test is described by the author as an adaptation of the Allport-Vernon Study of Values. Though scores on four separate scales are available, an overall score indicating total investment in interests and activities is also obtained. This test seemed particularly suitable for this study because in constructing it, items were selected using the method of equal appearing intervals so that they are weighed differentially. In other words, different items express different degrees of interest in any area. This makes it possible for one individual who has intense interest in one area to get a score as high as the individual who has broader but more superficial interests.

A narrow range of responsiveness theoretically, empirically, and even literally is perhaps the most general and precise description of constriction. This would be manifested by a relative curtailment of "free association" (4) and by a tendency to avoid extremes in expression of feeling. The Semantic Differential developed by Osgood (7, 8) can be used to measure these efforts. It consists of a series of concepts and sets of descriptive polarities, such that each concept is associated with each polarity. The polarities are at either end of a scale on which the subject places a mark to indicate his association of the concept with the polarity. The position of the mark indicates direction and intensity, with the central point indicating that the subject sees no relationship. Since each association involves a judgment as to the kind, as well as of the intensity of this relationship, the technique is well described as a combination of associational and scaling methods (7). As arranged for this study, the scales contain nine positions instead of the usual seven to make for a wider spread. The scales allow much room for individualized reactions to be

demonstrated in terms of direction and degree. The measure of constriction on this technique follows from the description of the constricted person as one with a narrow range of responsiveness. Highly constricted people would tend to have their marks cluster closer to the center of the various dimensions.

A group of items comprising a constriction scale was developed to obtain a measure of constriction based on the common clinical practice of describing a person as constricted. It consists of 42 statements which 29 out of 39 clinical psychologists agreed were descriptive of characteristics of constricted people. There was considerable variability in the use of specific items by various clinicians, but common use of a statement by 29 judges was required for it to be included. Thus in most instances the actual ratio of agreement would be higher than 29 out of 39. The universe from which the items were selected was the California Psychological Inventory developed by Gough. The items on the inventory were converted into the third person before being sent to the judges. The transformation into the third person served two purposes: (a) preliminary work suggested that it facilitated the task for the judge by preventing attempts to *predict* how the constricted person might respond; (b) it made it possible to use the scale as a peer description technique. Thus the judges selecting the items, and the subjects who later described each other in regard to them, worked from the frame of reference of an external observer. The ratings were obtained in a group session at which each person received five copies of the scale. On each copy appeared the name of one of the group members. Before beginning the administration a preliminary discussion was held, which was designed to develop rapport in the hope of persuading the group members to exercise care in making their judgments. Instructions were given orally at this time and were repeated after the scales were handed out. There were printed instructions at the end of each scale available for the use of the subjects as reminders. The subjects were to decide whether each item was primarily true or not about each person they rated. Ratees were assigned to each rater so that each person rated five different people and each person was rated five times. The final constriction index for each person was the median of the sum of the values assigned by the five raters.

The subjects in this study were 31 undergraduates, of whom 21 were members of a sorority, and 11 were male members of an athletic squad. In obtaining the peer descriptions each group was worked with separately. In the initial session, after some discussion of the study, the peer descriptions were obtained. Following this, appointments were made for administration of the three clinical techniques. Each subject was seen individually for these and, in most cases, all three tests were given in one session. The order of administration was the Rorschach technique, followed by the Bender-Gestalt test, and then the TAT. In a second group meeting the Semantic Differential and the interest test were administered.

Results and Discussion

Of the fifteen correlation coefficients only four are large enough to be statistically significant at the 5% level and the magnitude of these is low.

It would appear that the generality of constriction is highly limited. Though there seems to be some general constrictive tendency apparent in some people, much attention has to be devoted to the specifics of the situation within which a person is being evaluated. This finding seems consistent with the general emphasis today both in testing and in ego oriented psychotherapy to understand the real-

TABLE I—Intercorrelations Among the Constriction Measures

Measures	CPR	Ror.	TAT	B-G	S.D.
Constriction peer ratings					
Rorschach	.11				
TAT	.20	.34*			
Bender-Gestalt	.19	—.13	—.19		
Semantic Differential	.36*	—.18	.34*	.20	
Interests	.34*	.11	.00	.00	.17

* Significant at the 5% level.

ity situation of the client or patient before making "deep" interpretations. From the results of this study it might follow that the term constriction should be used, if at all, with great care. Precise usage would demand a specification of the tests or situations in which the constriction is noted. Even greater precision and utility might be gained by specifying the psychological functions that are constricted (6). Instead of saying that a person "is constricted" or has produced a "constricted Rorschach" it would be more helpful in understanding the individual were the dynamic and overt behavioral implications of such statements clearly articulated.

SUMMARY

This study was an attempt to explore the extent to which constriction is a general personality characteristic. After an analysis of some ways in which the concept is used in clinical psychology, measures representing these usages were selected and 31 subjects were tested. The scores on these measures, which included the Rorschach Test, TAT, Bender-Gestalt, peer ratings on a constriction scale, an interest test, and the Semantic Differential technique, were inter-correlated. The results indicate that very little generality can be attributed to the constriction concept.

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Hostile Acting Out and Rorschach Test Content[1]

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The presentations of Rorschach Test interpretations may be looked upon as working ideas, clinical hunches, and hypotheses based on an understanding of personality theory and clinical experience. Seen in this light, these hypotheses are amenable to experimental investigation and scientific scrutiny. Phillips and Smith (4) have been among the first to advance our progress in this area with their presentation of statements about relationships between Rorschach performance and other behavior. This paper investigates Phillips' hypotheses with reference to the anatomy content response in the Rorschach Test.

Phillips and Smith state the following: "Anatomy content reflects a sensitivity to, and concern with, the expression of destructive impulses. Paradoxically, those individuals who act out their destructive impulses do not develop anatomy content; the records of an assaultive group are conspicuously devoid of any anatomy responses." (4, p. 123)

PROBLEM

This hypothesis, suggested by Phillips and Smith, that individuals who act out their destructive impulses do not develop anatomy content in their Rorschach Test responses represents the first of two approaches utilized in this study of the problem of hostile acting out and Rorschach Test content. The specific prediction tested is that individuals who have a history of acting out produce less anatomy content in their Rorschach Test content than do individuals of similar psychological makeup who do not act out in hostile fashion.

PROCEDURE

The experimental population consisted of 37 male veterans who were hospitalized in a VA neuropsychiatric hospital. As part of the therapeutic team operations, each of these subjects had been administered a Rorschach Test by one of eight team psychologists and had produced ten or more Rorschach responses. Those patients whose protocols showed less than ten responses were excluded from the study. Other than the latter consideration, the experimental population was a random sample.

The median age of the subjects was 32, and the median education was eleventh grade. Diagnostically, 68% of the 37 subjects, or 25 in number, were classified as schizophrenic reactions, with over half of these (14) further classified as paranoid type. The diagnoses of the 12 non-schizophrenic patients were mainly psychoneurotic reactions and character disorder problems. Since Phillips and Smith point out that 800 of the 1590 Rorschach protocols to which they have made reference are from state hospital patients of varied diagnostic groups, the subjects utilized in this study appear to be consistent with the majority of patients upon which is based the hypotheses of the relationship between anatomy content and hostile acting out.

With respect to Rorschach data for the 37 subjects, the number of responses ranged from 10 to 99 per protocol, with a median of 28 responses. The number of anatomy responses per record ranged from 0 to 9, with

[1] Presented at the 1956 meeting of the Eastern Psychological Association in Atlantic City, New Jersey.

[2] The author wishes to acknowledge the assistance of Professor Austin W. Berkeley, Boston University, in the statistical treatment of the data.

[3] From research conducted while on the staff of Veterans Administration Hospital, Brockton, Massachusetts.

TABLE I. Rorschach Test Data from 37 Hospitalized Subjects Classified with Respect to Hostile Acting Out

Groups	Total R	Anatomy Content		Hostility Content	
Actors-out		No.	Classific.*	No.	Classific.*
1.	41	1	Low	11	High
2.	37	1	Low	4	High
3.	22	**2	Low	5	High
4.	13	3	High	2	Low
5.	17	0	Low	4	High
6.	18	1	Low	5	High
7.	19	**2	High	8	High
8.	20	0	Low	4	High
9.	22	0	Low	**3	Low
10.	23	5	High	0	Low
11.	33	1	Low	4	High
12.	35	**2	Low	0	Low
13.	37	1	Low	5	High
14.	42	1	Low	5	High
15.	45	0	Low	**3	High
16.	62	3	High	10	High
17.	68	9	High	7	High
18.	69	**2	High	**3	Low
19.	99	5	High	5	High
20.	10	7	High	1	Low
Non-Actors					
21.	43	3	High	2	Low
22.	81	4	High	**3	High
23.	13	3	High	0	Low
24.	13	0	Low	**3	Low
25.	13	1	Low	2	Low
26.	13	1	Low	1	Low
27.	15	0	Low	2	Low
28.	19	0	Low	**3	High
29.	21	**2	Low	1	Low
30.	25	**2	High	0	Low
31.	26	0	Low	2	Low
32.	28	3	High	2	Low
33.	30	3	High	1	Low
34.	32	3	High	0	Low
35.	32	4	High	4	High
36.	34	**2	Low	0	Low
37.	43	1	Low	5	High

* Classification as to high and low anatomy and hostility content based on whether score falls above or below median score of all subjects.

** Grand medians for anatomy and hostility were 2 and 3 responses, respectively. For those scores falling at the median, classifications were alternately classified as low and high.

the median at 2. A four-fold contingency table using 28 total responses and 2 anatomy responses as points of dichotomy resulted in a chi square of .23, with a p value of .52, supporting the contention that the classifications are independent of each other — i.e., that Rorschach productivity and production of anatomy content are independent phenomena. Looking at the data, one may see this reflected (Table I.) For example, the patient with the fewest number of Rorschach responses, 10, produced 7 anatomy responses, while another patient who gave as many as 43 responses produced just 1 anatomy response.

Keeping in mind that statistical treatment of the anatomy response distribution will utilize the median, one might note in passing that a mean production of 2.1 anatomy responses (standard deviation 2.0) in our experimental population is somewhat similar to Beck's et al. (1) report of 1.6 responses (standard devia-

tion 2.0) from among their sample of department store workers. Similarly, Rav (5) reported a mean of 1.6 anatomy responses from an unselected group of 200 normal male subjects.

To return to the specific prediction under test, individuals who have a history of hostile acting out will produce less anatomy content in their Rorschach Test content than do those who do not act out. To investigate this, the 37 subjects were divided into two groups. One group was designated the "Actors-out", totaling 20 subjects, and the second group was termed the 'Non-Actors", numbering 17. Categorization was based on an intensive review of these patients' overt behavior as recorded in social service and psychiatric histories, and on current hospital nursing and physicians' notes and progress reports. One example of a patient's history who was classified as an "Actors-out" may illustrate the basis for such classification. Mr. G. in the "Actors-out" group was reported in the case history, and from current social service reports, as having had a 3-year history of assaultive behavior towards his wife. He was reported to have made several murder attempts on her life, and on one occasion was said to have raped her. At the time he was picked up by police authorities and committed to a state hospital, he was reported to have unauthorized possession of a gun. Patients who were classed as "Non-Actors" were those who, from their histories or from current clinical records, showed no evidences of hostile acting-out behavior.

With respect to the two groups, the "Actors-out" and the "Non-Actors", there were no significant differences in age, education, diagnostic classification, or total Rorschach productivity.

Results

Investigation of the prediction that individuals who have a history of hostile acting out will produce less Rorschach anatomy content than do those who do not act out reveals no significant differences in production of anatomy content between the two groups. Utilizing deviations from the grand median of 2 anatomy responses as representing high and low anatomy content productivity, Rorschach protocols of the 20 'Actors-out" fell into classifications of 12 low anatomy respondents, and 8 high anatomy respondents. The 17 "Non-Actors" distributed themselves into 9 low anatomy and 8 high anatomy respondents, respectively (Table I). The chi square of .19 did not approach a statistical level of significance. Hence, it is concluded that Rorschach anatomy content productivity, considered in isolation to other Rorschach elements, does not differentiate the hostile "Actor-out" from the "Non-Actor" insofar as the latter characterizations are defined in this study.

Discussion

In view of this finding that anatomy content productivity in the Rorschach does not differentiate hostile "Actors-out" from "Non-Actors", a second approach to Rorschach content and acting out was utilized, taking into account several recent studies relating Rorschach content and acting-out behavior. In these studies, total content is assessed from the viewpoint of hostile connotation, and the relationship of this Rorschach assessment of hostility with other behavior is compared. For example, Storment and Finney (6) reported 23 non-assaultive and 23 assaultive hospitalized psychiatric patients differed significantly on a rating of aggressive content based upon total Rorschach content. As Finney (3) concluded, in a more recent report, these studies in general would tend to support the contention that hostility in the personality is reflected in the amount of destructive and hostile content in the Rorschach.

In light of the above, a possible explanation for the negative findings relative to anatomy content may be

found when one takes into account the probability that the "Non-Actors", not having impulses nor propensities directed toward hostility, are not concerned with necessities to control acting out behavior. Stating it another way is to think in dual terms; namely, hostile impulses, and control of them. It may be that the anatomy response represents, for those with the hostile impulses, a control factor represented by the channeling of these impulses into somatization and self-preoccupation, with its implications of concern over ones own bodily integrity as a possible fear-reaction because of fantasied retribution and retaliation for hostile thoughts and impulses. However, for those individuals who show little of these hostile feelings and impulses, the anatomy response may well represent some other meaning not necessarily associated with control over hostile impulsivity. There are, in fact, several hypotheses proposed in the psychological literature which suggest that anatomy content may have various other behavioral correlates.

If the above formulation is appropriate, the "Actors-out" and "Non-Actors" should, first of all, differ significantly in their so-called hostile content productions to the Rorschach Test. This is in fact the case. Utilizing a Rorschach score for hostile content similar to Elizur's (2) and, of course, omitting hostile-tinged anatomy responses in the scoring, the median number of "hostile" responses was 3. A contingency table (utilizing this median of 3 "hostile" responses as a cutting point for high and low categorizations) is presented in Table II for the "Actors-out" and "Non-Actors" in relation to their productivity of both anatomy and "hostile" content.

Employing Wilson's (7) modification of Rao's chi square statistic for a contingency table, an over-all chi square of 8.97 is obtained for both groups compared on hostile and anatomy response productivity which, with 3 degrees of freedom, is significant at about the .03 level. The interaction chi square between anatomy and hostile response productivity is not significant (chi square is .84) and, as mentioned previously, neither is the comparison between anatomy and groups (chi square is .19). However, the chi square between groups and hostile response productivity of 7.94 is significant at approximately the .005 probability level. Therefore, hostile response productivity on the Rorschach test is the major factor which

TABLE II. Contingency Table for Productivity of Anatomy and Hostile Content on the Rorschach Test

			Anatomy Responses		
			Low	High	
Actors-out	Hostile Responses	High	10	4	14
		Low	2	4	6
			12	8	20

			Anatomy Responses		
			Low	High	
Non-Actors	Hostile Responses	High	2	2	4
		Low	7	6	13
			9	8	17

	X^2	d.f.	p
Total	8.97	3	.03
Hostility	7.94	1	.005
Anatomy	0.19	1	Non-significant
Interaction of Hostility X Anatomy	0.84	1	Non-significant

differentiates the "Actors-out" population from the "Non-Actors".

The median number of "hostile" responses for the "Actors-out" group is 4, and for the "Non-Actors" is 2. The 'hostile" response is somewhat related to Rorschach productivity in that the more responses elicited, the more likely one is to produce a greater number of "hostile" responses. However, since this holds for both the "Actors-out" and the "Non-Actors", and since the two groups do not differ significantly on Rorschach productivity, it is appropriate to conclude that "Actors-out" produce a greater number of hostile content responses than do "Non-Actors", irrespective of total Rorschach responsivity. To review, it has been noted that "Actors-out" and "Non-Actors" do not differ in their production of anatomy responses to the Rorschach, but "Actors-out" do produce a greater number of so-called hostile percepts in their Rorschach responses than do "Non-Actors". Both findings are independent of total Rorschach responsivity.

Summary and Conclusions

To summarize, production of anatomy responses to the Rorschach does not differentiate "Actors-out" from "Non-Actors", unless propensities toward the production of overall hostile content are taken into account. Terming the latter activity as evidence of what might be called "hostile drive strength", one finds that high hostile drive strength along with few anatomy responses characterizes the "Actors-out". Those individuals with low hostile drive strength along with few anatomy responses characterize the "Non-Actors". This finding contributed to the results in the first portion of the study in which anatomy content considered in isolation does not differentiate between the "Actors-out" and the "Non-Actors". Therefore, the hypothesis that individuals who act out their destructive impulses do not develop anatomy content in their Rorschach Test responses, in not taking into account the individuals' hostile drive strength, does not hold up. However, when one takes into account the predisposition to hostile impulses — what has been termed 'hostile drive strength" — then one finds few anatomy responses are produced by the "Actors-out", who have high hostile drive strength, but also, few anatomy responses are given by the "Non-Actors", who are seemingly unconcerned with hostile impulses — who are low hostile drive individuals. Although a chi square reaches only a 10% significance level, the trend reveals that among the 22 of the 37 subjects so classified in the following dual terms, "Actors-out" are those with high hostile drive strength and few anatomy responses, while the "Non-Actors" show low hostile drive strength and a high production of anatomy responses to the Rorschach Test.

In conclusion, it is proposed that in relation to acting out of hostile and destructive impulses, production of the anatomy response be viewed as a channelizing or control activity for those individuals concerned with hostile impulses and tensions. But for those who do not have, or have a relatively small degree of these hostile drives within their personality organization, the anatomy response may not need to represent the control or substitutive activity required by the high hostile drive individuals. An investigation as to the possible meanings of anatomy response content in the Rorschach Test from respondents classified operationally as unconcerned with hostility and its control might well be worthy of consideration.

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BOOK REVIEWS

Garfield, Sol L. *Introductory Clinical Psychology*. New York: The Macmillan Company, 1957. Pp. xiii - 469.

Sometimes it is good fun to read a guide book to your own home town, to see how the city hall or the public library is supposed to look to the newcomer. It is an intrepid psychologist who undertakes to write a guide book to clinical psychology in this year of Our Lord 1957, when it is so difficult to lay the finger on what is stable in this rapidly changing and slippery field. Dr. Garfield gives the reader the impression that he has a bear by the tail. It is indeed in the department of understatement to say, as he does, that "the methods and activities of the clinical psychologist have not remained static" and that "the situation as it exists today is still far from conclusively settled." In this book which has been seven years in the writing and which has required "endless revision," there is a feeling of breathlessness about getting a paragraph completed before another drastic change occurs in this fluid, "dynamic and evolving profession."

This text is written at a level appropriate for college seniors who are considering a career in clinical psychology, or for graduate students in their first stages of training. The chapters deal with the expected subjects with which the young clinician would want to be familiar, under the rubrics of historical development, diagnostic study, appraisal of intellectual functions, methods of appraising personality, psychotherapy, research, clinical settings, problems and professional development. There would be other logical methods of organization, inclusion, and emphasis. There could be more on theory and so-called "methodology," less on professional problems; more on therapy, less on diagnostic study, and so on. But these matters are questions of taste and congeniality, and probably the proof of the pudding is in the eating. Actually we know as little about the "success" of college textbooks as about outcomes of therapy, and the problem is equally worthy of investigation.

To the old clinician, raised in an era before most of the present-day complexities were born, there are many interesting points about this book. I shall comment briefly on a few.

In spite of the fact that the problem of validation is not much more satisfactory in the measurement of intellectual functioning than it is anywhere else, less concern is expressed in this area and more feeling is conveyed of being on solid ground. This may be due in part to the longer history of clinical psychology in intelligence testing, in part to the greater reliability of the measures, in part to the tranquillizing effect of Terman's dictum of long ago that "intelligence is what the tests measure." Perhaps all of these have served to make us fret less about our difficulties with this criterion. In any case, in this text as in others, the sections on appraisal of intellectual functioning appear to be written with more comfort and confidence than those on the appraisal of personality.

If the book has a central doctrine, it is that "a mature understanding of the dynamics of personality adjustment is a prerequisite . . . for all functions of the clinician in psychology." This point is stressed again and again, and along with it is emphasized the student's need for vigilance that he do nothing blindly or routinely, but that instead he examine and criticize and understand and apply the theoretical assumptions that are implicit in any given technique. Thus he escapes the role of technician. The importance of theory as an organizing and integrating principle and as a frame of reference for all clinical functioning is always in the forefront of Garfield's discussion. He does not plug for one specific theory, though his own is easily discernible. Indeed he obviously does not regard it as part of his assignment to introduce the student to the broad field of personality theory. In considering theory, as in the description and elaboration of specific tests, he points to many references in the literature and sends the student out on his own.

Garfield sets a good example to the student as an honest critic. He faces the fact that our clinical instruments "leave much to be desired," and he encourages the student to assess rigorously their strengths and limitations. Even though he is frank about his own preference for projective tests, believing that they "have certain values which are not to be found in other tests of personality," nevertheless he deals candidly and sensitively with the chaos that results from their unreliability and from the unsatisfactory status of their validity, citing studies in support. This is not a painless operation and the absence of a solution makes it worse. Is personality itself

so variable and unreliable, and are the tests so sensitive that they detect the changes? Or are the tests merely unreliable? These questions are bothersome to Garfield and some of the negative findings on the validity of the Rorschach torment him, too.

Incidentally, it is interesting that although clinicians extol the value and importance of research, they are relatively impervious to the implications of research findings. Even writers who are not themselves partial to projective tests, after reviewing the discouraging evidence about their validity hasten to make such comments as "this is not to say that the instrument is of no value in the hands of a skilled clinician." It is demonstrated that certain kinds of Rorschach responses traditionally associated with anxiety fail completely to differentiate between anxious and non-anxious patients, or that other responses previously thought to differentiate between homosexual and non-homosexual groups do not do so. Yet clinicians seem to act and to write reports as if these findings were not known. I have wondered about the reasons for this curious phenomenon. Perhaps it stems from the fact that we have nothing better than these tests. Perhaps there is low faith in the criterion, so that there is little credence in the findings. Or perhaps we suffer from the lack of specificity in psychotherapeutic procedures growing out of test interpretation, a specificity which could confront us head on and prove us right or wrong.

Garfield makes a consistent effort to break down the association of "clinical" with abnormal phenomena. Hence he includes in the text a treatment of such overlapping functions as educational and vocational guidance, counselling and school psychology. He is trying to write this book with the question in mind, "Where is clinical psychology going?" Clearly he feels it might go almost anywhere. He discusses these problems only superficially, not exhaustively or systematically, but with the belief that the clinical psychologist should be prepared, should have some familiarity with these tests and with these fields, and should view an educational or vocational problem as a probable symptom of some deeper emotional difficulty.

The discussion of psychotherapy is well tied in with the professional status of clinical psychology, with full awareness of the interprofessional whirlpools and cross currents. There is no minimizing of the tensions and obstacles that lie ahead, and the issues that are considered are very new and current, hot off the griddle.

It is in the chapter on research that Garfield's frustration is expressed most clearly. He deplores the triviality of much current research, the negative findings when experiments are repeated, the difficulty of studying basic problems and the consequent reluctance, the poor quality of many published studies from the point of view of research design and method. Here is the frustrated lament of a clinical psychologist pressed by feelings of guilt that our discipline does so poorly, of desperation that where we need to know so much we know so little.

A very weak feature about this book is the subject index. There is no entry for such a concept as "sample" or "sampling"; "theory" does not appear there; nor does "questionnaire," although all of these are mentioned in the text. This deficiency may limit its usefulness to students.

It is fascinating to compare this very fresh textbook in clinical psychology with Louttit's of 1936, the earliest in the field. In Louttit there is no word of psychotherapy. There is one brief chapter on "personality problems" in which inferiority feelings, jealousy, fear, daydreaming, negativism and disobedience are considered. But there is nothing about personality measurement or theory. There is a great deal about school retardation, specific disabilities and sensory defects. Then clinical psychology was frankly an art and there was no effort to make it a scientific discipline and no worry over methods and criteria. The world of clinical psychology was limited and there needed be no space given to ethical principles or to a variety of settings for the clinician. Already, however, there was a struggle for a definition of clinical psychology and a committee of the APA had been appointed to formulate one! The life of the clinician was simpler in those days and much less frustrating, but today's hurlyburly offers far more stimulus and promise.

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We deeply regret the death of Dr. Ruth Tolman on September 18, 1957.

Levine, Edna S. *Youth In A Soundless World: A Search For Personality.* New York: New York University Press, 1956. pp. xiii + 217.

Levine's study is a welcome addition to the meager literature on personality processes in the physically handicapped. It raises what, to this reviewer, is the critical issue in studies of such groups: Is the impoverishment and flattening of performance a function of the handicap itself or of the type of schooling which society has invented for deaf children? Levine's study of Wechsler and Rorschach performance of 31 adolescent girls in a New York residential school for the deaf confirms the findings of personality deficiencies that others have also noted in the deaf:

> The personality pattern of the deaf subjects was found to be characterized by: (1) pronounced underdevelopment in conceptual forms of mental activity; (2) emotional underdevelopment; (3) a substantial lag in understanding the dynamics of interpersonal relationships as well as the world about; (4) a highly egocentric life perspective; (5) a markedly constricted life area; and (6) a rigid adherence to the book-of-etiquette code rather than inner sensibility as standards for behaving and even for feeling. (p. 146)

This devastating conclusion rests on the Wechsler and Rorschach findings. The Wechsler evidence shows that, despite normal Full Scale I.Q.'s in this selected group, the deaf girls exhibit clear-cut quantitative inferiority in all Verbal subtests and superiority on the Performance Scale, particularly "in visual-motor coordination and visual organization ability," especially in the Object Assembly and Block Design subtests. Levine interprets the high Performance results as evidence of high "innate mental capacity" with the Verbal deficit seen as pointing to "correspondenly delayed pace of abstract mental development."

Unlike Fiedler and Stone (this journal, 1956, 273-279) in their study of children with mild hearing defects, Levine reports for her severely handicapped deaf girls an array of marked shifts toward inferiority in Rorschach performance: underproduction of W with mediocre quality, overproduction of D, underproduction of M, overproduction of F, underproduction of FC coupled with high CF, overproduction of A and an "absence of traditional Rorschach signs of emotional disturbance." While Levine's Rorschach analyses may be regarded by some as over-literal there can be no quarrel with her basic finding that "the functioning intelligence of the Experimental Group is seen to be of the noncreative, reproductive type with lowered capacity for conceptual thinking and strong emphasis upon concrete tangible stimuli," at the same time that there is "no indication of limited mental endowment." (p. 142) The reviewer would add that the typical Rorschach scoring patterns produced by these 15 to 18 year olds are remarkably immature and resemble those of young school age children.

For this reviewer the crux of the matter comes in Dr. Levine's cautious interpretation which points to the techniques of teaching as well as to the handicap of deafness itself.

> . . . because of the unique restrictions imposed by deafness upon global and unconscious learning, reality is taught object by object, incident by incident, situation by situation in a more or less rigid way. It is a very long time before such labored learnings become assimilated from the consciously retained form into the deeper and more personal spheres of inner development. In the meantime, deaf pupils are obliged to hold fast to conscious standards, to the rigid scheme of reality they have been taught, and having so been taught, about which they are sure. It is to be expected that such a rigid scheme of acquiring inner concepts would result in constriction and inflexibility of personality. (p. 136-7)
>
> By now, the public is generally familiar with what happens to speech when deafness is early and profound. But few stop to think of what happens to the development of language when it is learned the way the deaf do: fragment by fragment, slowly, painstakingly, in contrived situations, often out of context, and usually out of rhythm with the growing child's needs of the moment. (p. 164)

In addition to these rigidity-producing characteristics of the teaching methods used with deaf children one should consider the isolation of children in special schools, particularly residential schools. Levine speaks of this in terms of protection:

> The general absence of signs of disturbance may be due to the fact that the residential school environment to which these girls have adjusted protects them from numerous daily-life experiences that might cause tension and anxiety. It is a question whether such protection makes for inner stability or for weakness when the time comes to face the world. (p. 144)

It is a further question whether such "pro-

tection" does not make for further constriction, narrowing of experience and, hence, rigidity.

All in all, however, the problem that the deaf child faces is, as Levine suggests, partly the product of our attempts to bridge the communication gap. As she puts it, "the deaf child's need for vocabulary is so great that the push to fill him with words instead of language is often irresistible." (p. 144) The reviewer shares Levine's respect for the skill and devotion of the teachers of deaf children, but it is his belief that there is room for even more questioning of the pattern of their teaching than Levine here ventures. Indeed, some as yet unpublished research in which the reviewer has participated has brought forth experimental evidence that some loosening of these shackles is possible; evidence which shows that with broader experiences as part of their educational background deaf children may go further toward "spontaneous ventures into not-yet-learned and consequently unknown life areas." (p. 144)

Dr. Levine's volume includes a section on educational approaches to deaf children which psychologists will find most valuable although the material may be quite familiar to specialists in the education of the deaf. By the same token, the rather detailed description of test materials and procedures which may induce impatience in the psychologist may well be of value to the educators for whom the book is also intended.

L. Joseph Stone

GENERAL NEWSLETTER

Pertejo, J. Estado actual de los tests proyectivos y el problema de su validacion. *Revista de Psicologia general y aplicada,* 1956, *11,* 663-675.

A discussion of sources of information, points of view and considerations in the validation of the Rorschach primarily, including the state of affairs in Spain regarding the Rorschach.

Boulanger-Balleyguier, G. Ètude sur le C.A.T.: influence du stimulus sur les recits d'enfants de 3 a 8 ans. *Revue de Psychologie appliquée,* 1957, *7,* 1-28.

A research investigation into the relative importance of stimulus characteristics and internal dynamics as determinants of stories told to the C.A.T.

Tamarin, Georges. Un essai d'explication des reponses "position" dans le test de Rorschach. *Revue de Psychologie appliquée,* 1957, *7,* 53-58.

Rorschach position responses are conceived as a combination of exaggerated precision and confabulation and imply a struggle toward reality.

Grünewald, Gerhard. Graphometrie Grössenveränderungen einer 25 Jahre umfassenden Handschriftenwicklung. *Zeitschr. f. diagnost. Psychol.,* 1957, *5,* 81-100.

Methods of evaluation and measurement of handwriting are described and exemplified by a 25-year follow-up of one person's writing.

Bäumler, Fr. Psychodiagnostische Untersuchungen bei einem 12-jährigen schizophrenen Mädchen. *Zeitschr. f. diagnost. Psychol.,* 1957, *5,* 114-122.

The Rorschach and Tree Tests are used in clarification of the diagnosis of a schizophrenic girl.

Yosinaru, Huzioka. A statistical approach to group comparison based on the distribution of Rorschach responses. *Memoire of the Research Institute for Humanistic Studies*: Kyoto University, 1957, *1,* 23-38.

A mathematical formulation is presented which attempts to treat the whole Rorschach pattern of a cultural group as a unit. The distributions of location-content frequencies in a variety of Rorschach samples are found to be similar to distributions found in biological studies.

Sacco, Francesco. Il P.F.S. di Rosenzweig "Form for Children" applicato ai fanciulli siciliani: patterns e tendenze. *Rassegna di Psicol. generale e clinica,* 1956, *1,* 75-90.

Statistical findings are presented for the performance of a sample of Sicilian children on Rosenzweig's P-F test.

Ponzo, Ezio. Proposta di una tecnica di controllo del test del disegno di persona. *Rassegna di Psicol. generale e clinica,* 1956, *1,* 91-104.

A variation of human figure drawing procedures requiring the subject to draw as if he were an idiot.

Seguin, Roger. Interpretacao estrutural de sintoma psicopatologicos atraves de testes projetivos. *J. Brasileiro de Psiquiatria,* 1957, *6,* 26-41.

A case study of an obsessive-compulsive neurosis with the Rorschach and TAT.

The President's Report

Let me bring you up to date on the affairs of the Society for Projective Techniques, in case you were not at the meetings: It would be easier for me to chide those of you who were absent from the business meeting, had I, myself, been a more faithful attendant of the business meetings of the various societies I belong to, in the past. Having this job now with the Society for Projective Techniques has taught me something about that: It is not very wise to cultivate a lofty disdain of business meetings and "political" things in general, reserving one's interest for pure science. The two aspects are as inseparable as form and content: unless the administrative and executive machinery provides the best possible framework, professional and scientific progress cannot be optimal.

An all-day meeting of the *outgoing Board* was followed by the *annual business meeting;* the highlights of the problems of the past year were presented and the recommendations by the hardworking committees and the Board were submitted to vote for decision. For the present, I will single out only a few details: the Research Committee under Dr. Feifel did most valuable work in outlining research tasks for the Society; the Regional Committee, under Dr. Dörken, was very active in stimulating local interests in projective techniques; the Nominating Committee, under Dr. Holzberg, suggested some procedural changes for elections, which you will probably consider marked progress; the editorial staff of our Journal, I sincerely believe, does a much better job than any other scientific Journal I know of, with regard to little publication lag, utilization of space and general caliber. Lastly, the work of the Training Committee, under Dr. Kass, was of very great value: They collected data from thirty Universities concerning their practices in teaching projective techniques in doctoral programs.

One alarming idea was expressed by some of those responsible for training in projective techniques, namely, that they consider it the University's job to offer only fundamental methodological considerations rather than to teach the use of projective techniques. This seems like a bad case of washing one's hands of definitive problems, via rationalization. The matter hinges on the fact that psychology was primarily an academic science and has become primarily a profession, which is, however, still taught in graduate schools rather than professional schools, or both. Since candidates in a clinical Ph.D. program obviously expect to be professional clinicians, a University appears to me, in such cases, to fall short of offering the necessary training when it proposes to offer only a purely academic program. The result is far from academic — an uneven, if not poor level of competence in dealing with projective techniques and other clinical problems, the knowledge of which must be acquired on a catch as catch can basis, after the Ph.D. The universities, I personally think, behave that way because they do not have enough properly trained clinicians on their staff to teach clinical procedures properly. Where, at least, this involves projective techniques, I believe it is the Society's task to look after the matter.

So much for the very excellent work of the various Committees; I will return to the past work of the other committees in future reports of relevant developments, but now let me tell you more about the meetings: The Program arranged by the Society for Projective Techniques and Division 12 seemed to be an unusual success: Most of the time the sym-

posium filled the largest available auditorium—which was very large indeed. The presidential dinner and address was well received. Even the hotel food proved better than feared and the surreptitiously carried bicarbonate of soda, unnecessary. So many people came to hear Dr. Klopfer that the address had to be moved to a larger auditorium. Some of the introductory remarks I had planned for a modest sized group, feeling cozy after their culinary repast, could not be made to the large and somewhat air-conditioned crowd. Dr. Klopfer, however, adapted admirably and delivered a most stimulating paper on the Rorschachs of cancer patients. I'll confine my scoop to saying that it seems the less psychodynamic defensiveness, the more benign the cancer and vice-versa, (as if psychological defenses might decrease the somatic resistance). At the business meeting, a resolution had previously expressed special appreciation of Dr. Klopfer's services to the Society over the many years, and in many ways.

The *business meeting of the incoming Board* met in a properly smoke-filled hotel room, without any further resemblance to a political gathering. In fact, a good deal of work was done, viz:

1) It was voted to establish an ad hoc committee to study the problems of nominations and elections.

2) The Board voted to cooperate with the Committee on Primary Records of the National Research Council in making the necessary steps for obtaining funds, first to explore what data might be useful to micro-film and then to proceed forthwith. Raw data of Ph.D., thesis were considered possibly particularly useful. Bertram Kaplan and Theodora Abel were appointed co-chairmen of a committee on micro-filming of projective data, to work out the details of selection, manufacture and distribution. I am particularly happy about this program, which ties in with another pet plan of mine, which was also voted into existence, namely,

3) A Research Registry (as part of the Research Committee) whose function will be:

a) To advise on Ph.D. thesis, when requested to do so

b) To integrate related material and

c) To stimulate research in needed areas of projective techniques.

It was suggested that the committee begin its work by eliciting reactions from faculty members in various institutions, to the idea of their being able to draw upon the Registry for suggestions, and for information on both available and also badly needed data in projective methods. The idea is to suggest some common frame of reference for collection of material, to have basically comparable data in the long run. It was also suggested that the Registry solicit funds to carry on its work.

Hand in hand with the strengthening of the research aspects of projective techniques must go the better training in them, already mentioned. As a small Society, the Society for Projective Techniques can appropriately function as the guardian of the optimal use of projective methods in the ever-enlarging and often uncharted field of psychology. For instance, it is essential that the optimal time in the training of a clinical psychologist be determined for teaching projective techniques. As already stated, it is also essential that only experienced clinicians, well-trained in projective techniques, teach these methods.

Ruth Monroe and Michael Finn were appointed co-chairmen of this Committee with these ideas in mind. It was specifically voted that they suggest to the Education and Training Board of the American Psychological Association the advisability of considering fellowship status in the Society of Projective Techniques a requirement for teaching projective techniques in approved clinical pro-

grams. In this way, the Society could properly safeguard the interest of all psychology in ascertaining the best possible training in this vital clinical area.

It is obvious that the Society will be happy to consider Fellowship status for all those who teach projective techniques and speedily open its ranks to all who are qualified.

It may seem regrettable to erect further barriers and rules and guilds and forms of approval. Personally, I even dislike the regimentation of red and green traffic lights. Likes and dislikes, however, have nothing to do with necessities (in a technologically increasingly complex society with increasing interaction) to safeguard us all against individual irresponsibility.

With this take-off from Freud's thoughts on *Civilization and Its Discontents,* I will close my report. Maybe interstellar space will open up new frontiers for more happily irresponsible living—such as testing Martians with psychodiagnostic devices dreamt up in glossy magazines.

LEOPOLD BELLAK, M.D.

ANNOUNCEMENTS

RESEARCH EXCHANGE

EDITH WEISSKOPF-JOELSON

As you remember this column was started for the purpose of (1) publishing research ideas and (2) exchanging rare projective records.

Here is a contribution to the former purpose: Would a pictureless TAT be diagnostically useful? In other words, if subjects would be asked to tell stories without any objective stimulus, how would these stories compare in diagnostic value to traditional TAT stories? In investigating a problem of this type what would be the best way of comparing the two sets of stories as to diagnostic value? Would we hypothesize that they yield not only a different amount but also a different kind of diagnostic material? What theoretical implications would such research have regarding the problem of stimulus ambiguity? Would the relative efficacy of the pictureless TAT vary as a function of the subjects' personalities? If so, what kind of hypotheses could be formulated regarding such variations? How would an investigation of this problem be related to previous research?

A reader of this column might be interested enough in this question to do some empirical research on it. Or some readers might want to elaborate further on the problem in the next issue of the JPT. Others might like to express their own ideas in the Research Exchange. Thus, this column might become a place where the speculator and the active researcher meet, for the benefit of psychology, we hope.

Mr. Richard Benjamin, Director, Cold Spring Institute, Cold Spring-on-Hudson, New York, has sent us the following contribution:

We have projective records on thirty women, ranging in age from 55 to 80 years with a mean of 65 years, and in IQs from 119 to 145, with a mean of 130. The batteries administered to these subjects consist of a Rorschach, a Sentence Completion Test (Miale-Holsopple), a Mosaic Test, a Wechsler-Bellevue Scale, a Bender Gestalt Test, and, with some subjects, a TAT. The records are available to anyone who wishes to use them for research purposes.

BOOK REVIEWS

The Editors have had some difficulty finding reviewers for a variety of books which seem relevant for review in this journal. Because of the uncertainty and slowness of answers from persons who have been invited to write reviews, the Executive Editor has decided to announce the titles of books that require reviewers and allow our readers to volunteer.

We request that you do not volunteer to review a book unless you (1) will do it within three months of receipt of the book (2) will be moderately objective in your appraisal (3) are fairly sophisticated in the area dealt with and (4) know the language pretty well. Reviewers may retain the book. Here is what we have at present.

Brower and Abt: *Progress in clinical psychology* (Vol. 2).

Buhler and Manson: *The Picture World Test.*

Burton and Harris: *Clinical studies of personality.*

Klopfer et al: *Developments in the Rorschach technique* (Vol. 2).

Kragh: *Actual-genetic model of perception-personality.*

Kutash and Gehl: *Graphomotor Projection Technique.*

Libo: *The Picture Impressions Test.*

Meili-Dworetzki: *Das Bild des Menschen in der Vorstellung und Darstellung des Kleinkindes.*

Minkowska: *Le Rorschach.*

Phillipson: *The Object Relations Technique.*

Piotrowski: *Perceptanalysis.*

Stern, Stein, and Bloom: *Methods in personality assessment.*

Stumper: *Triebstruktur und Geisteskrankheiten.*

Tomkins and Miner: *The Tomkins-Horn Picture Arrangement Test.*

van den Broek: *Behn-Rorschach Test.*

Please address requests to Bertram R. Forer, 2170 Live Oak Drive, E., Los Angeles 28, Calif.

ANNOUNCEMENT

The editors of the *Journal of Projective Techniques* regret the omission of the sample cartoon from the paper by Shapiro, Biber, and Minuchin: The Cartoon Situations Test: a semi-structured technique for assessing aspects of personality pertinent to the teaching process, *Journal of Projective Techniques,* 1957, *21,* 172-184. The cartoon has been printed and copies can be obtained from Dr. Edna Shapiro, Bank Street College of Education, 69 Bank St., New York 14, N. Y.

NEW MEMBERS

The Society for Projective Techniques is happy to welcome the following newly elected members:

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